**GChronD**

Interactive comment

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

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Response to Reviewer #1 of Schoene et al.

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 a section of text written by the reviewer and an A: at the beginning of a response paragraph. 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.

A: We thank the reviewer for his/her comments on our manuscript. In addition to minor editorial comments, we highlight here that the reviewer is suggesting adding more

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about the original data interpretation of the Ar/Ar dataset. Given this and the comments of Dr. Renne, we are proposing to add a section to the paper that summarizes the details of the methodology and decisions made by Schoene2019 and Sprain2019 that result in the eruption ages from each paper.

R: This is a follow-up paper discussing the two contributions that appeared 2019 in Science (Sprain et al., 2019; Schoene et al., 2019) discussing the age and emplacement mode of the Deccan Trap flood basalts, and compared it to the age of impact (=age of KPg boundary). This work is about (i) precision and accuracy of the Ar-Ar and U-Pb techniques; (ii) propagation of uncertainties; (iii) fundamental assumptions of age-depth model calculation; (iv) temporal resolution of isotope dating methods.

R: I definitely endorse publication of this manuscript; after publication of the two above mentioned papers in Science, side by side, the specialized as well as the non- specialized community is kind of waiting for a profound discussion of all the factors that influence the final result of each method and herewith define the scientific statement. I would like to acknowledge that the manuscript is written in an extremely concise and correct way. R: Detailed comments line-by-line: line 43: sounds like before the Phanerozoic there have been no LIPs

A: The sentence will be tidied up to make the intended point more clear.

R: line 51: Kasbohm et al. is definitely not sufficient as a reference. I would suggest one of the papers of Burgess for the S-LIP, and of Svensen et al. (2012) for the K-LIP.

A: done

R: Line 53: here you only mention magmatic volatiles. However, many people think that thermogenic $SO_2$ and $CO_2$ are much more important drivers of climate change (Svensen papers, Sobolev SLIP). This also implies that the main trigger would be the intrusive part of a LIP, causing contact metamorphism of evaporite and/or organic matter bearing lithologies. This hypothesis is supported by the fact that some LIPs do not

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have profound environmental impacts, and do not crosscut such critical lithologies. In this sense, correct estimation of the extrusive/intrusive ration will become very important!

A: agreed, and this has been clarified.

R: line 94: I have a memory that there is a study directly dating impact spherules in the Chicxulub crater by Ar-Ar??

A: not to our knowledge, but Renne et al., 2013 and Renne et al., 2018 date tektites and spherules from Haiti and Gorgonilla island, respectively, and show those dates match the Ir layer in Hell's Creek, MT, using Ar-Ar in sanidine, to within tens of kyr. We're having trouble finding why this comment was made about line 94, however.

R: line 124: different LIPs show variably depths of erosion, either the basalt flows are mainly exposed (as is the case of the so far dated part of the Deccan) or the sill-dyke complex is mainly preserved and the basalt flows removed (as is the case of the Karoo LIP. In either case, the volume of the lacking part is very difficult to estimate.

A: True, we have added wording to note this distinction.

R: line 134: just asking myself whether reproducing an entire figure with caption from another journal is allowed? Since they are "slightly adapted", would there be a benefit of re- drawing them?

A: We checked, and it is allowed due the licensing agreement of AAAS.

R: line 145-147: I don't really see the point of this sentence here: this is true for any diagram containing "rates".

A: No, it is not. The sentence: "Note that while this is a more realistic depiction of the eruption rates derived from the 40Ar/39Ar data, this plot has difficulty taking into account the non-negligible uncertainties in formation boundary ages and therefore eruption rates". The point is that if the volume vs. height model is correct, then the proba-

bilistic estimate of eruption rate derived from the geochronology and the stratigraphy is correct. But when one draws lines between formation boundaries, it adds a false precision because there is uncertainty in the age of those boundaries that is hard to depict in a figure such as Fig. 3. One could smear out the lines, or, as in Fig. 3, leave actual numbers for time off the x-axis. The reality is that any probabilistic estimate of eruption rates is hard to depict because of the covariance that arises from the constraint of an assumed total volume – i.e. if eruptions are really fast in one place, then have to be slower in another for a given MC realization. This goes beyond the scope of this paper perhaps but is fun to think about and try to visualize better.

R: line 153: this is Keller et al. (2018) Geochem. Perspectives line 167: "the MCMC algorithm used above . . ." Is this sufficiently characterized, just citing Keller et al.? For the general understanding, a few works would help, especially making the difference between the Keller approach and Bacon?

A: This is a good point, and one that comes up from Dr. Hodges' review as well. In addition to the Keller et al. Geochem. Perspectives paper you mention, the code itself (Keller 2018) is a citable open-source code package on Github and OSF, which includes a detailed description of how the model works. But we can't expect the average reader to go digest this stuff. We tried to highlight what we think is the main difference later in the manuscript (assumptions about deposition rates), and we would consider adding a more detailed comparison if necessary.

R: line 187: I am not entirely sure about my following statement: I have in mind that Bacon allows to change the priors and to vary the memory/linearity term quite freely, whereas Bchron (you don't mention) can't. Maybe the authors check again this statement with the original Blaauw and Christen paper.

A: In using Bacon and applying it to the Deccan datasets, we have found that the deposition rate prior, while changeable, has very little effect on the outcome – the result is always a very linear sedimentation rate if the input data permits it to be linear.

Bchron (not explored in this paper because it was not used either dataset originally) is much less restrictive in terms of priors in sedimentation rate.

R: line 200: "similar" – is more dispersed, isn't it?

A: Yes, it is more dispersed, but not dramatically so. However, as Dr. Sprain pointed out in her review, the uncertainty inputs in the model of the Ambenali ghat section were incorrect. The result from fixing that is that the position of the KPB in the Ambenali ghat section (Fig. 5) is in fact very similar to that of the entire composite section dataset (Fig. 4).

R: line 219: these are single collector data from a MAP spectrometer, which may possibly not be as precise as the present-day state of the art is. However, I am not in the position to make a quantitative statement here.

A: The dataset, to our knowledge, were collected entirely on the MAP spectrometer, but it is not clear to us whether on a newer multicollector instrument whether higher precision would be achievable or whether it will still be limited by very low K contents in plagioclase. We have modified the sentence to refer specifically to the published dataset, so as to not imply the lower precision in a fundamental limitation to Ar/Ar geochronology of plagioclase.

R: line 227-229: You refer to your figure 6. "no uncertainty" and "$\pm$1Ma$\pm$ is not what you show there, but $\pm$10kyr and $\pm$270kyr.

A: Yes, the intention of that statement was to indicate that we needn't plot those extreme endmembers because the outcome is so obvious. We modified the wording of these sentences to make it more obvious that the $\pm$0 and $\pm$1 Ma cases were mentioned as a thought experiment to introduce the results for the less obvious cases.

R: line 235: put directly 270kyr here, not "70kyr less than. . ."

A: Done. But note that following Dr. Sprain's review, we adopt a mean uncertainty of $\pm$220 kyr not $\pm$270 kyr.

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R: Comment post-line 251: I think that you stay very correct and nice here, not to attack the Sprain et al. paper, maybe too much? I personally have additional concerns: 1) the argon data are done on multigrain fractions, without demonstration that the diffusional parameters of the individual, analyzed plagioclase grains are indeed identical and can be treated in bulk. Assuming this would mean that every single plagioclase has the identical number of twin planes and/or exsolution planes per cubic unit. It is a matter of fact that the argon community is not checking for the mineralogical and crystallographic homogeneity of the sample material. The plateaus do show signs of weak Ar loss (which is correctly removed from the plateau calculation of course) and also show some minor signs of steps that may have a recoil component (?). 2) to increase precision, Sprain et al have averaged several multi-grain plateau ages into one weighted mean age for one sample level in order to increase precision. Statistically speaking, probably questionable. I could see that Schoene et al., may also like to comment on these statistical approaches and discuss their validity?

A: Following this comment, and the comments of Dr. Renne, it seems there is some demand for detail about both the Ar/Ar and U-Pb datasets themselves. Both papers made assumptions that result in the proposed ages being "model dates". The concerns expressed by the reviewer above are good examples of assumptions that go into weighted mean dates of large multi-grain aliquots (or even, but less so, weighted means of a population of single grain dates). Dr. Renne pointed out his concerns about the assumptions that go into the model dates of the U-Pb dataset. In the revised version of the manuscript, we propose to add a new section, section 2, that will run through these assumptions for both datasets in an attempt to provide the reader with the reality that both datasets, if not all geochronological dates, are model dates based on assumptions of variable robustness.

R: line 275: "influence" is not the right term, maybe "directly translate to, propagate into". . .

A: Good point. We chose the word "control."

R: line 301: this means that the correct error propagation has not been done on the argon data set. I would see that this fact needs to be more prominently to be pointed out, because otherwise you are comparing apples and pears. I think that age models have to be based on identical error treatments, otherwise any comparison and any scientific conclusion is obsolete.

A: Well, one could argue that the uncertainty propagation for the Ar dataset is correct, and that the FCs age from Kuiper et al. is the one in error; noting that study had no implications for the 40K decay constant or physical constants, and therefore if the (ca. 1-2%) uncertainties in those were included, everything would overlap. The point with this section, which perhaps the reviewer is getting at, is that there are disagreements in the Ar community in these values that are far larger than any difference between the Deccan Ar/Ar and U/Pb datasets as published. So if one focuses on the place in the datasets (upper Ambenali) that actually disagree beyond the published internal uncertainties, you could spend a lot of time arguing about the geology, or other possible reasons, for nothing.

R: lines 313: Same comment as above, magmatic volatiles alone are not the drivers of climate change, many people consider thermogenic gases more important. Is not in the major focus here but would need a sentence to add this (Siberian traps, Karoo). If the thermogenic gases are more important, then the major driver are the sills and dykes! That's actually what I think, and therefore the volume of erupted basalts may have a minor role in the whole discussion of the driver of mass extinction!?

A: As above, this is good point. In this case, the sentence as written, which points to both intrusive and extrusive magmatism, has that covered.

R: Line 355: here it comes! I think you downplayed tis before, this idea has to appear from the beginning of the argumentation I feel.

A: Perhaps the added section 2 will address this point, but we will also revise the beginning of the paper to address the comments of Dr. Sprain, who states that the

main point of misconception that we raise in this paper, Fig. 4 from Sprain 2019, was unintentional. Given that plot was apparently never intended to convey eruptive flux, despite the description of it, changes the playing field a bit.

R: Line 360 As a general comment to chapter 6: to make it easier to digest for non-specialist, structuring the main points into bullet-points would be maybe better?

A: We will consider this restructuring, but meant it initially to be a bit of a narrative compared to the abstract, which lists the main points.

R: Line 639: I agree that the graph shows km3, but through the calibration of their age model it becomes an eruptive flux, too. The fig. 3 (line 641) is just redrawn from the Sprain data. I somehow feel that there is too much importance put to this discrepancy (but I agree on the fact that the graphs are misleading).

A: Not totally sure we understand this one. Fig. 4 of Sprain2019 does not become eruptive flux by plotting total volume of a formation versus the time it took to erupt. Whlie it was part of the point to put emphasis on Fig. 4 from Sprain2019 as a source of misconceptions in the community about how to interpret these datasets together, we can reword parts of this text in response to Dr. Sprain's review to acknowledge this was not the intention of their paper and focus on the other parts of our analysis.

R: Some final comments: What are now the overall conclusions? The authors could go even further than they do, if they wanted: The non-equal treatment of uncertainties between the two dating techniques leads to some disparate scientific conclusions (e.g., pulsed vs. continuous). One of them may be wrong, but definitely none of them can be proved with the present data set, as they state. The geochronological community should put an end to non-equal reporting practices between the Ar-Ar and U-Pb sub-communities. This also implies that, reconsidering some of the systematic uncertainties, the data set in Sprain et al. seems to be over interpreted. By adding some kind of an outlook, the paper may gain leverage?

A: It is true that the authors of the original papers take different approaches towards calculating depositional ages of beds/flows. The newly added section 2 will point out some of the main differences between the way ages are generated and reported. But in the end, we are happy to accept in this paper that each approach involves assumptions that need to be tested in future work. The main point is not that either paper mistreated how they calculated the dates of individual beds/flows. The main point is that even if both papers report accurate dates, readers have walked away with the impression that there are large differences between the datasets, and their simply aren't. Perhaps the main reason for this is Figure 4 from Sprain et al. (2019), which is titled eruptive flux, but doesn't have units of flux and was apparently not intended to be interpreted as such (see review of Dr. Sprain). But it was, and that needs to be clarified. We are also adding a bit of text in the discussion that will provide more detail on a number of things that could be done to test the proposed eruption rate models (e.g., pulsed versus not), since the current datasets cannot do that.

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