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

Anonymous Referee #1

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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.

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.

Detailed comments line-by-line: line 43: sounds like before the Phanerozoic there have been no LIPs 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. 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 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! line 94: I have a memory that there is a study directly dating impact spherules in the Chicxulub crater by Ar-Ar??

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. 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 redrawing them? line 145-147: I don't really see the point of this sentence here: this is true for any diagram containing "rates". 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? 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. line 200: "similar" – is more dispersed, isn't it?

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. line 227-229: You refer to your figure 6. “no uncertainty” and “±1Ma± is not what you show there, but ±10kyr and ±270kyr. line 235: put directly 270kyr here, not “70kyr less than…” 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? line 275: “influence” is not the right term, maybe “directly translate to, propagate into”… 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. lines 313 ff: 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!? Line 355: here it comes! I think you downplayed tis before, this idea has to appear from the beginning of the argumentation I feel. 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?

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).

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, re-considering 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?

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