**Re-Review of "An evaluation of Deccan Traps eruption rates using geochronologic data" by Schoene et al.**

The revision of "An evaluation of Deccan Traps eruption rates using geochronologic data" by Schoene et al. is nicely implemented and I appreciate the authors' work adjusting the manuscript per the concerns outlined in my initial review. I think this version of the manuscript does a nice job of highlighting the similarities and differences (mostly similarities!) between the 40Ar/39Ar and U/Pb datasets for the Deccan, in addition to helping correct some of the misinterpretations of my admittedly poorly labelled Figure 4 in Sprain et al. (2019). Overall, I think this is an important contribution that will help to clarify many of the misconceptions about Deccan geochronology. It further nicely highlights some of the steps forward toward reconciling the existing datasets and improving Deccan chronology beyond what was achieved in our recent publications.

I did find a few errors and additionally have a few minor comments that I have included below.

Please don't hesitate to reach out with additional questions.

Thanks,
Courtney Sprain

**Edits:**

Line 77: Cut the parenthesis before "(Beane et al., 1996;"

Line 123: Change "Ar-Ar" to "$^{40}Ar/^{39}Ar$" to be consistent with rest of text.

Line 177: Our multigrain aliquots did not contain 10^3 grains. It was more on the order of 10's to max ~$10^2$. Please correct.

Line 218-220: I would appreciate this being corrected to say something like "Figure 2 is the correct plot showing eruption rate, but however, as acknowledged by Sprain (2020), the poor word choice on Figure 4 has led to confusion suggesting that this figure plots eruption rate/flux." In Sprain et al. (2019) we specifically show our calculated eruption rates that we cite in the text in panel (B) on Figure 2.

Line 231: Not to sound like a broken record, but our calculated eruption rates for Wai and pre-Wai (including age uncertainties) are shown in our Figure 2 panel B. For clarity for readers citing estimated eruption rates from Sprain et al. (2019), it would be useful if you could cite that the calculated eruption rates (with uncertainties) used in our manuscript are shown in our Figure 2 and that readers should refer to this, and not attempt to estimate rate from our Figure 4. We did not show age uncertainty in Figure 4 as the main goal of the figure was to show the correlation between timing of eruptions and climate change.

Lines 184-186: This statement isn't accurate. First, the plagioclase grain size used in our study was sufficiently large that we can ignore the effects of Ar-recoil (see Jourdan et al., 2007, 2014).

Second, yes it is possible that subtle open system behaviour occurred, but it is unlikely to affect ages within the stated uncertainty. Further, we did acid leach our samples, which should have removed any minor alteration. I would reword this sentence to "However, it is possible that unresolvable subtle open system behaviour due to alteration or Ar-loss may have occurred."

Lines 186-188: This statement is also inaccurate. It is very important that I point out that in Renne et al. (2015), the plateau age we produced that was precise, concordant, and inaccurate was from whole-rock groundmass, NOT from a plagioclase separate. This is important to note because the whole rock analyses have two issues that were mitigated by using plagioclase separates. First, the groundmass is finer grained and more prone to alteration than plagioclase. Second, the grain size for the groundmass was significantly smaller than that of the plagioclase such that in the sample analysed we saw major effects from Ar-recoil. This is not an issue in the plagioclase separates as the grain size is significantly larger and well above the range calculated in Jourdan et al. (2007, 2014) where recoil effects need to be addressed. It's also important to note that the inaccuracy of age in this sample is most likely due to the recoil effects (which can be observed by the high-age slope in the first few incremental heating steps). This effect is not something we expect nor observe in our plagioclase separates, and as such it is inappropriate to equate the results from that sample to our ages determined from plagioclase separates. Additionally, relating to the differences observed between Barry et al. (2013) and Kasbohm and Schoene (2018), I would not attribute that to inaccurate, precise, and concordant data in Barry et al. (2013), but instead due to the fact that Barry et al. (2013) was a compilation paper of the Ar data available for the CRB that ranged in date of study over many years. It additionally included both whole rock and plagioclase separate ages. The authors in Barry et al. (2013) did their best to choose the best data available at the time, but were still limited due to the data not being produced using modern 40Ar/39Ar analytical methods. I strongly suspect that if a new study performed in the CRB was done using modern 40Ar/39Ar methods on plag multi-grain aliquots, that the results would agree with the U/Pb data. This is obviously conjecture at this point, but to support my suspicion, I've included here a plot of the best 40Ar/39Ar data for the Deccan produced before Renne et al. (2015) and Sprain et al. (2019). As you can see, the data is very scattered and cannot easily be used for age analysis. However, when we re-did the study using modern analytical techniques, on mulit-grain aliquots of plagioclase, you can see we were able to vastly improve the data, obtain stratigraphic superposition, and ages that generally agree with the U/Pb dates. Please modify accordingly.

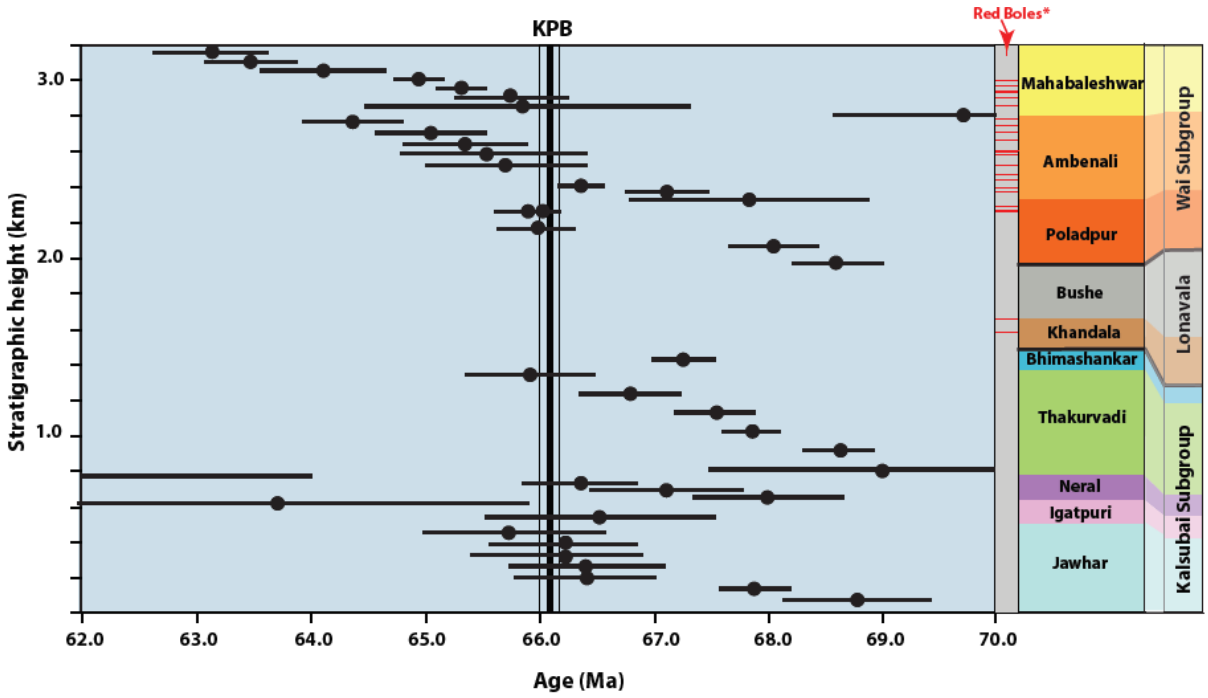


Figure 1: "Best" 40Ar/39Ar dates from Deccan before Renne et al. (2015) and Sprain et al. (2019). Recalibrated to Renne et al. (2011) and plotted at 2 σ uncertainty.

Lines 172-192: I don't entirely follow the criticism of the multi-grain technique here. As you note, unlike Pb, Ar is degassed from plag at low-T's and based on diffusion models, should be degassed prior to eruption. Yes, there could be subtle alteration (but likely removed via our acid leaching protocol), or loss (but not recoil, as mentioned above), but this is not likely to bias our ages within the precision of our analysis. The multi-grain technique on plag is widely used in our community and to my knowledge, there is no indication nor studies suggesting that there are major issues with it. We could run the plag one by one, but we wouldn't be able to check for nuances of alteration, recoil, or open-system behaviour by doing so, which the step-heating technique allows us to do. I'd argue the biggest problem with our dataset is we're limited in precision due to the K-content of the plagioclase. This is something we have no control over and unless we find sanidine in the red boles, we are unlikely to vastly improve the precision of the 40Ar/39Ar data in the Deccan.

Line 288: I'm still not reproducing your average 40Ar/39Ar precision of ±220 ka. The uncertainties that should be used are the ones listed in Figure 1 of Sprain et al. (2019), as this shows the combined data from Sprain et al. (2019) and Renne et al. (2015). I get an average uncertainty (2-sigma) of ~213 ka. Here's my calculation:

(0.134 + 0.100 + 0.168 + 0.072 + 0.164 + 0.144 + 0.134 + 0.204 + 0.258 + 0.184 + 0.164 + 0.302 +0.152 + 0.094 + 0.168 + 0.638 + 0.200 + 0.130 + 0.166 + 0.208 + 0.158 + 0.308 + 0.496 +0.362 +0.206)/25 = 0.21256 Ma

Please modify here and in Figure 6. It doesn't change anything, but the correct number might as well be used!

Figure 2: Make sure to receive copyright permission to use the figures from Science.

Figure 3: I think this figure would benefit from adding numbers to the time axis, or at a minimum the chron boundaries (so people don't have to jump back to figure 2).

Figure 7. I appreciate the effort the authors put into doing the analysis for figure 7. But, the figure is a bit hard to follow. First, it would be useful if you labelled the formations. I know you used the same color scheme throughout, but I found myself having to go back to other figures to remind myself which colors went with which formations. Second, could you explain in the figure caption (or text) why you're plotting Sanhagad fort and Katraj Ghat, and Mahabaleshwar Ghat and Khambatki Ghat on the same graphs in b)? Are you confident they are close enough together such that the elevations between the sections are comparable (noting that Jay et al. (2005), noticed many meters of variation in the placement of the C29r/C29n boundary around Mahabaleshwar). I'm sure they are every close together and it's fine, but stating that in the text would clarify it for readers. Finally, I am really confused as to how you're building the composite section. What do you mean "Elevation relative to Ambenali Ghat"? How was this calculated? Additionally, in the text you state they are superimposed onto Mahabaleshwar Ghat. Do you mean Mahabaleshwar Ghat instead of Ambenali Ghat on the figure axes? Are the sections supposed to be plotted at their relative elevations in a) ? And why do the formation boundaries in the composite plot shift in elevation in between a) and b)? You also state, "To generate Fig. 7c we moved samples vertically until they fell on a line defined by the dates from the Mahabaleshwar Ghat while maintaining superposition in individual sections." Wouldn't this necessitate each formation having the same thickness everywhere? We know this isn't the case, at least based on Jay et al. (2005)'s study.

I'm sure I'm being daft on some things here (start of semester chaos has limited my brain power). But, if I'm confused, others may also be confused, so it might be worth explaining this figure in a little more detail.

Fig. 8. Quick question, which decay constant did you use in your recalibration? Renne et al. (2010), Min et al. (2000), or Steiger and Jager (1977)?