Response to the reviews by AE Dr. Mark and reviewer Dr. Sprain

Dear Dr. Mark, please find below responses to the comments made by yourself and Dr. Sprain. I believe we've been able to accommodate all the suggestions and hopefully adequately clarified the areas of confusion pointed out by Dr. Sprain. Thanks for your feedback and continued attention to our manuscript. You will find the original comments in black and the responses in blue.

AE Comments:

Only other consideration I have for your Deccan paper is: Fig. 7 shows (clearly labelled) arbitrary width green and red horizontal bars for FCs K08 and R11. I would suggest you make the width of these bars the full width of the uncertainty associated with both K08 and R11 and extend these lines horizontally past the Bayesian eruption ages calculated using Keller 2018. At the levels of uncertainty shown (2-sigma full systematic for Ar/Ar? 2-sigma analytical for single crystal U-Pb? 2-sigma systematic for the U-Pb using Keller algorithm? Include in figure caption) the R11 FCs age overlaps with youngest zircon, touches the MELTS and bootstrapped eruption age, and clear overlap with the uniform prior age.

In discussion that the FCs age of R11 is not consistent with the U-Pb age for FCs this figure is going to confuse a lot of readers.

We added the extension of the uncertainty bars to the figure to make the comparison with U-Pb dates easier. We also emphasized in the figure caption and the text that these estimates for FCs do overlap at the 95% confidence level when systematic uncertainties are considered. Hopefully the point gets across that when systematic uncertainties are included, any high precision comparison between the datasets is not permitted at this point.

Sprain comments:

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;"

Done

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

Done

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.

Thanks, I'm glad this was corrected. I simply took the 40 mg quoted in Renne et al and estimated an average grain size, which was an underestimate.

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.

I added similar text to clarify this.

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.

I added text noting that our recalculated rates agree well with those in Fig. 2 from Sprain et al. 2019

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."

I went ahead and changed the wording to that suggested, but will note that the Jourdan study (2014 had plag aliquots) show that plagioclase measurements on a standard did not show signs of recoil to within about a percent. This is different than measurements on unknowns with their own geologic complexities (cracks, alteration) that may complicate Ar-recoil. And, the plagioclase data in Sprain et al., 2019, are more precise than that, so assessing the effects of recoil at that level of precision is difficult. With regard to acid leaching, I question whether there are enough (any?) studies that can independently evaluate how effective it is, so it's hard to say whether it is a problem or not.

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.

We acknowledge and completely agree that plagioclase is superior to groundmass/glass (based on many comparisons between groundmass/glass dates with both U-Pb dates and Ar/Ar dates of single mineral separates that convincingly show the groundmass/glass dates to be inaccurate). But it has yet to be independently demonstrated that plagioclase dates are accurate to the level of precision reported in Sprain et al. (2019). We could be wrong, but a literature survey shows that the plagioclase data in Sprain et al. (2019) is a factor of two more precise than any previous study, and that's a big difference for the task at hand. It is different to suggest in theory that the plagioclase dates should be accurate, than to have demonstrated that over and over again.

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.

There is no doubt that the data in Sprain et al. (2019) and Renne et al. (2015) is a vast improvement over older K-Ar and Ar/Ar data, which hopefully comes across in our paper. We agree that carefully produced plagioclase data for the CRB would likely agree with the U-Pb data. Regardless, the statement about the Barry et al. (2013) compilation does not refer the numerous old Ar/Ar dates from the CRB, it refers to the brand-new data produced for that study using modern techniques (concordant groundmass step-heating data) that was consistently inaccurate by up to or more than a million years. Regardless, this paragraph is not saying the Ar/Ar data is wrong, it's taking the opportunity to point out that despite our best efforts, there are remaining uncertainties that are difficult to quantify. This paper is not going to spend time pointing out what those uncertainties are for U-Pb (as the text now does, at the request of a reviewer) and not do the same for Ar/Ar.

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.

The criticism is simple: in every example we can think of in geochronology, when more precise data is obtained, more complexity in the minerals we date is observed, and it becomes clear that this complexity suggests combining many grains together can lead to overly precise results. This is true for U-Pb, Rb-Sr, U-Th/He, Lu-Hf, Sm-Nd, and Ar/Ar for sandine, and it will be true as we continue to microsample zircons to get smaller domains. We should learn from these lessons and be wary of repeating them. The point is that it has yet to be shown that data as precise as in Sprain et al (2019) are accurate at that level of precision, so we can't just assume it is because in theory there shouldn't be anything wrong. Again, we're not talking about major issues, we're talking about subtle differences here, which could be important at this level of precision.

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!

Sounds good. It looks like there are some differences that resulted from a) one case where the uncertainty in the figures disagree with the data table (BOR14-1) and one case where data from Renne et al. 2015 disagree with that reported in Fig. 1 (I used the number from Renne et al 2015). But yes, these are small discrepancies. The figure has been updated to use only the uncertainties from Fig. 1.

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

They don't require it, it turns out.

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).

Good idea, we put the chrons on there.

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.

Done.

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.

Good point, we explain this now a bit in the text and a bit in the caption (the elevations were dip corrected in the case of Sanhagad and Katraj, and in the other case it was just convenient to put them on the same plot to save space).

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?

Sorry, this is just nomenclature. We have always called it Mahabaleshwar ghat instead of Ambenali. I've tried to use Ambenali throughout because that seems to be the preferred, but was sloppy in this figure. Also tried to make it clear in the text how the figure was made. It's really just drawing a line by eye on the Ambenali ghat section, then placing the other horizons on it based on where the measured eruption date falls on that line.

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.

We note this is confusing and have tried to be explicit in revising so it is clear what we have done. We expanded the figure caption and added a bunch of text to the section to describe step by step what was done to make that figure. We note it's not supposed to be quantitative in any sense, but simply to illustrate what would be necessary to adjust the existing stratigraphy to get linear eruption rates with our data.

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.

Definitely, you're not being daft, and hopefully this is clearer now.

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)?

I think I just changed age of FCs in Cam Mercer's recalculator, left decay constant as Renne, but honestly I don't remember. If that's what I did, I realize this is not strictly correct given Kuiper's paper didn't use Renne, but the difference is miniscule (ca. 10 kyr) and since it's just plotted, not put in a table, I declare it doesn't matter given the overall shift of 210 kyr.