Geochronology Discuss.,
https://doi.org/10.5194/gchron-2020-11-AC4, 2020

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

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Received and published: 13 October 2020

Response to Reviewer 3, Paul Renne 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. A: We thank Dr. Renne for taking the time to comment on our paper. We must admit that it is highly unusual to receive a review that is predominantly about a previously published paper, rather than the new manuscript at issue, but we welcome the opportunity to discuss these details in an open forum such as is provided here. Because there are few substantive comments made about the submitted manuscript, our responses have not resulted in any changes to the manuscript yet. However, we are open to adding a section to the paper

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Discussion paper

that outlines the assumptions that go into each dating method, which would address the overarching concern of Dr. Renne (about U-Pb), and that of reviewer 1 (about Ar/Ar). We are also open to adding a discussion about questioning the stratigraphic correlations from the literature that we used in Schoene2019 as a way forward to test the pulsed eruption model, as this seems like the most appetizing way to make the pulses go away. Both of these additions would substantially change the scope of the paper. We wait to make those changes following the AE's response/recommendation.

R: Contrasts between the interpretations of Schoene et al. (2019) and Sprain et al. (2019) have garnered attention, and an objective appraisal of the reasons for these contrasts would be a useful contribution to a fairly large community. Unfortunately, this manuscript does not really accomplish that - in fact, it comes across as mainly a comment on Sprain et al. (2019) with essentially no acknowledgement of the limitations of Schoene et al. (2019). Consequently, my review is to some extent a review of Schoene et al. (2019), because many factors that must be considered in an objective comparison of these two papers have not been addressed.

A: Our goal of this manuscript was not to provide a holisitic comparison of the datasets presented in Schoene2019 and Sprain2019. There are obviously many geologic and analytical factors, some of which Dr. Renne brings up below, that require consideration when attempting to address why two geochronologic datasets do not agree. But our main point of this manuscript is that the datasets, despite such complicating factors, in fact agree quite well – and figuring out where they agree and disagree is contingent on first treating the dates produced in a correct and consistent way. It is our impression, from published papers, conference talks, (anonymous) reviews of other topically-related papers, and conversations, that the geologic community believes these two datasets disagree starkly with one another, which simply is not true. The main reason for this, we believe, is summarized in Figure 4 of Sprain2019, which is labeled as eruptive flux, and readers thus logically interpret as eruptive flux. As we have shown, and as has been corroborated by Dr. Sprain in her review of this manuscript, Figure

4 of Sprain2019 simply does not plot eruptive flux. This reality therefore needs to be clarified to the community such that we can move forward to ask important questions about the geologic framework and chemical stratigraphy of the Deccan Traps used in both Sprain2019 and Schoene2019; the analytical details and choices made in each study; and how depositional ages are interpreted from dispersed (in the case of U-Pb) and imprecise (in the case of Ar/Ar) datasets.

R: I have raised many of the points to follow in conversation with some of the authors, but evidently my concerns have not been taken seriously. Thus, I think it will be constructive to stimulate some open discussion, which this forum provides in principle.

A: We would like to disagree that we have not taken seriously Dr. Renne's comments on our dataset, and on U-Pb datasets in general. Over the last decade, his recognition of pre-eruptive zircon growth and its effect on age interpretations has been very important in helping to guide the U-Pb community towards more robust age interpretations, for which he is thanked. We hope that as Ar/Ar datasets become more precise and accurate over time, and begin to resolve similar dispersion in their single-crystal datasets (as they are beginning to do for sanidine populations), that we have helped contribute to the groundwork for how to interpret eruptions ages from complicated and dispersed age populations. For the Deccan dataset in particular, we are carrying out ongoing work to address the points raised by Dr. Renne, which will be described below.

R: I think it is critical that readers don't interpret this manuscript as a head to head comparison of the two dating methods. The two studies being compared measured very different things and made very different assumptions, and the comparison can't be confused with a referendum on either technique.

A: We fully agree, which is in part why in this manuscript we steered away from getting into some of the issues raised below that get into the weeds of comparing the two datasets, given depositional ages determined by both methods are based on a series of interpretations and assumptions.

R: To begin with, a balanced treatment of both papers would have to consider the fact that neither data set- nor interpretations drawn from them- can be said to represent the Deccan Traps as a whole, and comparing the two in detail, as done here, requires tremendous faith in the notion that the chemical stratigraphy used to demarcate formations can be assumed to be chronostratigraphic.

A: While we share the notion that the stratigraphy of the Deccan Traps, as defined based on field geology, petrology, and chemical stratigraphy, should be treated as a work in progress, we must admit that we have been surprised at how well it has held up to the test of geochronology. Thus, it does not require tremendous faith. It serves as a testable hypothesis that has largely held up to the scrutiny of Sprain2019 and Schoene2019, requiring much kudos to those workers who provided this foundation for us to stand on. In fact, every sample we collected, with the exception of the two noted below, fell within the relative age sequence predicted during sample collection in the field. As we outline below however, our dataset does permit the existence of numerous problems in the stratigraphy, especially if one strives to make the hypothesized hiatuses disappear.

R: Worse yet, to apply this assumption to redefine a formational contact (Poladpur-Ambenali) by âĹij100 m, as done by Schoene et al. (2019) is clearly circular.

A: This is the one place in our dataset where the units as mapped, showed a hiatus within a formation, based on the U-Pb data. It is fine with us if other workers prefer to keep the boundary as was originally mapped, in which case the proposed hiatus would fall within the Ambenali instead of at the Poladpur-Ambenali contact. We are of the opinion that it made more sense given the U-Pb dataset to package all the flows that erupted very quickly into distinct packages that work very well with the pre-defined formations, especially given the geochemical distinction between Ambenali and Poladpur is negligible. It is not obvious to us that there need be distinct Ambenali and Poladpur Fms. If the two Fms were grouped into one, it could be used to change the eruption rate model in Schoene2019 considerably because one could shift the relative stratigraphic order in our composite section more easily.

R: The assumption of isochronous chemically defined formation contacts has recently been challenged by Kale et al. (2020). Sprain et al. (2019) did depict their results on a figure showing cumulative volume versus age, but reserved any quantitative inference (via age-modelling) for a single section, requiring no faith in chemically-defined formation contacts being regionally isochronous.

A: We have to push back on this a little. The cumulative volume versus age plot in Sprain2019 was used to calculate average eruption rates across the entire Deccan Traps, which was (somewhat ambiguously) used to speculate about an increase in eruption rate at the KPB. The same approach was the entire basis for the conclusions of Renne et al. 2015, who used a quantitative analysis in this way to argue there was a dramatic increase in eruption rate as a result of the Chicxulub impact.

R: Lava flow fields are constructive features that build uneven topography, which then controls the distribution of subsequent flow fields. This poses a major caveat for age models based on a composite of six sections to infer a complex volumetric extrusion rate.

A: We acknowledge this, and have therefore been pleasantly surprised that the chemical stratigraphy has held up so well. It could be, that in a flood basalt province as large as the Deccan Traps, that flow trajectory migration occurred on a much larger scale than one observes in less voluminous outpourings such as observed in, e.g., Hawaii. It could be that, if our hypothesized hiatuses hold up in the Western Ghats, that eruptions were in fact continuous and the predominant deposition direction was to the east (where outcrop is worse) or west (where outcrop is under water).

However, and this response pertains to much of the text above, if one wanted the hiatuses to go away, the best way to do it is through questioning the stratigraphic correlations. As we outline in subsequent responses below, we don't find the arguments for our dates and hiatuses resulting from zircon residence time or detrital zircons very compelling. The following argument seems more likely.

The interested reader should probably have Figs. 1 and S2 from Schoene2019 in front of them. For the Mahabaleshwar-Ambenali hiatus, one could make the hiatus disappear by arguing that RBO, and all other samples from the Sanhagad Fort section, is not upper Ambenali as previously mapped in Beane et al., 1986 (note the color coding of samples from particular sections in Fig. 1), but is instead lower Ambenali. Because we did not recover zircons from any redboles in the upper Ambenali in the Mahabeleshwar Ghat (aka the Ambenali Ghat), this shift would produce a sampling gap in our compilation, essentially between RBB2/RBAY and RBE. The implications of this change would be that either a) the Ambenali Fm in the Sanhaghad Fort section is very thick and the Ambenali-Mahabaleshwar contact is incorrect as mapped (i.e. the Mahabaleshwar Fm is not actually present at the top of that ghat), or b) that there is an unconformity/hiatus locally in the Sanhaghad Fort section just above RBO (perhaps this option is supported by the geochemical break used to define the Ambenali-Mahableshwar contact in that section by Beane et al, 1986). One test of this would be to do paleomag through the Sanhaghad Fort section to see if the C29r-C29n reversal is present there, which should appear in the lower Mahabaleshwar Fm. This scenario would still require a hiatus without any physical evidence, but it could be local.

Making the Poladpur-Ambenali hiatus disappear is slightly trickier, but if enough of the existing mapping and chemical stratigraphy is wrong, it is possible. That hiatus largely depends on the dates from RBX from the Mahabeleshwar Ghat being at the Poladpur-Ambenali contact and the date from RBBS being in the upper Poladpur. The geology of the Supe Ghat east of Pune where RBBS was sampled is less certain compared to the other sections in that there are two publications (Khadri et al., 1999; Duraiswami et al., 2014) that draw Fm boundaries in very different places and disagree whether or not the Bushe Fm is even present. So perhaps that geology is uncertain enough that one can argue that RBBS is actually lower Poladpur. If RBX were also lower Poladpur rather than uppermost, then the hiatus would go away while keeping everything in the

Katraj Ghat and Sanhagad Ghat as mapped (or even if you make the adjustment suggested above to make the Ambenali-Mahabaleshwar hiatus go away; note the relative stratigraphy between these two ghats is hard to adjust because they are located very close to each other and essentially continuous). Placing RBX in the lower Poladpur would require the lower Poladpur to be very thick (300+ m) in the Mahabaleshwar Ghat given the obvious geochemical distinction between the Bushe and Poladpur Fms. This in turn would require either an unconformity above RBX such that the middle and upper Poladpur are not present (with no physical evidence) or that the Poladpur is very thick and the Ambenali is very thin in that ghat. This scenario still requires a large outpouring of lava (in this case the lower Poladpur instead of most of the Poladpur) shortly before the KPB.

So in summary, it is possible that the hiatuses documented in Schoene2019 are an artifact of the existing stratigraphic correlations. It would require the existing mapping and chemical stratigraphy to be pretty wrong in numerous places, but given the ambiguity in the geochemical distinction between the Ambenali and Poladpur Fms, this doesn't seem impossible. We of course looked into these options prior to Schoene2019 but in the end felt it was simpler and more consistent to honor the existing geologic framework, and found that perhaps the most interesting finding in Schoene2019 was that the Poladpur erupted very quickly and began slightly before the KPB, and this conclusion doesn't change with the rearrangement of the Poladpur-Ambenali Fms hypothesized above.

We are currently attempting to test the possibilities with additional sampling but have thus far failed to return zircons from critical samples, e.g., in the upper Ambenali Fm of the Mahabaleshwar ghat.

R: On this topic, Sprain et al. (2019) are criticized for using a conservatively parameterized age model. Let's acknowledge that no age models are truly objective, and Sprain et al. (2019) chose to minimize the degrees of freedom in the absence of evidence to guide such choices. Ironically, applying the Keller age model to the data of Sprain et al.

(2019) in Figure 4 (upper panel) reproduces quite well the most probable accumulation history of Sprain et al. (2019) for the one stratigraphic interval we modeled. This would appear to validate the conservative choices we made in our Bacon model.

A: Certainly any age model would reproduce the same average depositional rate, and this would be similar for a simple least-squares fit of the data. Our assertion is that the Bacon model underestimates the uncertainties in ages of any particular horizon because it imposes an effectively linear deposition rate through the modeled stratigraphy. Despite the rhetorical positioning thereof, there is nothing "conservative" about requiring deposition to be continuous and linear when both observation and common sense show that most sedimentary (let alone volcanic) deposition is characteristically discontinuous, unsteady, and nonlinear (e.g. Sadler 1981; Sadler & Jerolmack, 2015; Thorardson & Self 1993). Evidence that this assumption is affecting the results in this particular case is that the Bacon model presented in Sprain2019 results in a date for the C29r-C29n magnetic reversal in the Ambenali ghat that does not agree with that from Hell's Creek in Montana produced in Sprain2018 and cited in Fig. 4 of Sprain2019. This inconsistency may be the result of overprecision from weighted mean dates of plagioclase, the Bacon model, or both (among other possibilities).

R: The point is made in several places (e.g., lines 218-219) that the lower precision of the Ar/Ar dates somehow inhibits detection of the pulses inferred by Schoene et al. (2019). Yet Figure 4 (lower panel) seems to show the contrary- uncertainties in the Ar/Ar data are clearly small enough to detect such pulses (a) if they are province-wide, and (b) if they are even real. Further to this point, it is obviously appropriate to include systematic uncertainties in comparing the two methods, but not when determining relative ages as in whether or not the Ar/Ar data permit the existence of strong pulses. The most probable inference from the Ar/Ar data is that there are no eruption pulses within the Wai subgroup, at least as recorded in the Ambenali Ghat section.

A: We disagree with this, for the reasons outlined in detail in the manuscript. We don't see at all how Fig. 4 (lower panel) illustrates how the Ar/Ar data can resolve the pulses

– if they are real, the Ar/Ar data cannot see them. Illustrating this is the purpose of the modeling exercise using synthetic data summarized in Fig. 6. We show in that figure, and describe in section 4, how increasing uncertainties restricts the ability to resolve pulses in eruption, or any inflection in deposition rate. There are interesting parallels to be made with the sampling frequency required to resolve periodicities in time-series analysis (e.g., the Nyquist frequency), but we won't go into that here.

R: But to my mind the most significant flaw in this manuscript is that it fails to acknowledge the limitations of the data underlying Schoene et al's interpretations. The first limitation arises from interpreting zircon ages as the ages of eruptions that occurred between the emplacement of lava flows above and below the red boles from which they are extracted. It is entirely an assumption that each of the zircon populations used was produced by an explosive pyroclastic eruption yielding a pyroclastic fall deposit that occurred during an interlude between successive basalt flows, and was deposited in the nascent paleosol $\pm$ alluvium $\pm$ eolian material $\pm$? that the red boles represent. This assumption is completely unvalidated and is not even discussed by the authors beyond the acknowledgment that these are ". . . PRESUMED (my emphasis) ash-bearing intervals" (line 77). The fact that this is a presumption is ignored in the subsequent discussion. Every red bole I've examined contains a component of detrital material. The presence of much older zircons in some of the populations (even Proterozoic) also may signal detrital input, although they could also be xenocrysts yielded by an explosive eruption.

A: As noted at the outset of this response and in the manuscript, it was not our intention to dig into geologic or analytical issues of each dating method given the need to first address what can and cannot be said about eruption rates, taking the depositional ages as correct. To be blunt: however unintentional, Sprain2019 made mistakes in plotting their data – or in how the descriptions of those plots characterize how to interpret the data – that misguide readers. Regardless of original intent, Figure 4 of Sprain2019 has been widely and entirely incorrectly interpreted as an "Eruptive flux", as it is described in the figure caption. Once such errors are addressed and the datasets are treated in the same way (as this manuscript does), it is then appropriate to ask questions about where they differ, where they are the same, and whether or not factors that went into determination of depositional ages could have caused this. We therefore don't see a lack of discussion of the dating approach used in the U-Pb dataset as a valid criticism of this manuscript. It is, however, a valid criticism of the U-Pb age model in Schoene2019, and we are happy to expand on that in this response, and in the paper if the editors deem this appropriate.

Only a handful of our samples can be shown to be direct ashfall deposits in the field, while most are weathering horizons inferred to contain volcanic input, so it is valid to ask whether or not the zircons retrieved from most of the redboles record deposition of ashfall. Here's our reasoning for thinking this is not a bad assumption:

1) Geology: These are horizons between basalt flows on top of a gigantic shield volcano. Shield volcanoes have a huge footprint and significant topography. Indeed, one of the most impressive topographic features on Earth, the big island of Hawaii, is a tiny shield volcano in comparison to those that must have been created by LIPs such as the Deccan Traps. To transport detrital zircons up a shield volcano to be deposited in the redboles seems difficult mechanisticly. To do so in some way that only incorporates zircons that preserve stratigraphic order in the basalt flows is even less probable, especially given that the most likely age of detrital zircons in the region is not an age spanning the KPB, because the basement rocks are Proterozoic to Archean and the sedimentary basins are Paleozoic to Mesozoic. There are about 8% xenocrystic/detrital zircon grains in the redboles that span that age range, but the remaining 90+% are consistent with the stratigraphic ordering predicted for the basalts. Hence, for these zircons to be detrital, they would need to be sourced locally from rocks that have the right age to preserve stratigraphic order. The basalts themselves do not contain zircon, we and others have looked. Thus, if the zircons found in redboles were not direct ashfall, they must have been locally sourced from ashfall. It is difficult to envision

a way this would work, but it seems hard to rule out without additional arguments.

2) Stratigraphic continuity: The possible scenario outlined above would need to work such that the resulting zircon populations could be used to generate depositional ages (using any viable method – the Bayesian approach we used, using the youngest grain, etc., see supplementary material in Schoene2019) that matched that predicted by stratigraphic continuity in every single case. This is by even qualitative consideration an extremely unlikely scenario.

3) Zircon populations: if the zircons found in redboles were detrital as opposed to derived from ashfall with negligible reworking, one would predict that the populations would not be distinct from one another. For example, take the sequence of redboles in the Poladpur Fm that nominally have the same depositional age (as modeled by the Bayesian approach we used, or by taking the youngest zircon, etc.) and hence led to the argument that this Fm was erupted rapidly. For the detrital argument to work, you'd need a pile of zircons uphill from the redboles that is tapped throughout the eruption of the Poladpur without input from new zircons/ash that give younger dates. When the Ambenali Fm started erupting, you'd need to switch to a new pile of uphill zircons that is tapped throughout the eruption of the Ambenali. In such a case, you'd expect the population of detrital zircons from each Poladpur redbole to be the same (and showing no signs of transport such as rounding etc.) physically, geochemically, and in terms of age spectra. In some cases, the zircon populations are indistinguishable based on these criteria. In other cases, they are quite distinct. For example RBP (see discussion below) contains zircons that are completely different from any other redbole – tiny and low U. RBX (top of Poladpur) contains zircons with remarkably reproducible ages, i.e. few antecrysts compared to other redboles. An additional test, which we have recently carried out and will be publishing soon, is the zircon geochemistry and Hf isotopic compositions from the same zircons dated. We won't spoil those results here.

It is worth noting that none of these lines of evidence is foolproof of course, but neither is anything in geochronology, so we must continue to test the hypothesis, as we are, that populations of zircons in redboles can be used to obtain reliable estimates of depositional ages.

R: Absence of abrasive rounding of zircons is not evidence of no residence time in surficial environments- for example, there are plenty of perfectly euhedral and angular Cretacous zircons from Sierra Nevada granitoids found in Neogene sandstones hundreds of miles away.

A: we don't know of the zircons referred to here, but would be curious to look at them and see if they pass our definition of euhedral.

R: More fundamentally, interpreting highly dispersed (relative to analytical precision) zircon age distributions to infer eruption age is not straightforward due to magma residence time effects. The authors are well aware of this phenomenon and have worked valiantly to model their way out of this problem, but it is unclear whether the model works in the case of potentially mixed populations or that it accounts for the fact that even individual zircons record 10's of ka growth histories (e.g., Ickert et al., 2015). Moreover, there are still relatively few studies amenable to validating the model in different magmatic regimes and/or when subtly older inheritance from another source is present to perturb whatever distribution the juvenile magmatic population has.

A: Dr. Renne is correct that dispersed zircon data is a fundamental challenge that needs to be addressed in arriving at a best estimate eruption age from zircons in volcanic deposits. So for this response let's assume the zircons in redboles are in fact volcanic in origin with little reworking. It is not accurate to say we are trying to model our way out of this problem. As discussed in the supplemental info of Schoene2019, whether you use the Bayesian model, just interpret the youngest zircon as closest to eruption, or take a weighted mean of the youngest several, the conclusions of that study are the same. The more important point is that the petrology of zircon crystallizing in a (closed system) magma requires that once zircon is saturated it will continue to grow until eruption, and so any approach to estimating the time at which zircon stopped crystallizing is more accurate than, say, a weighted mean of many zircons that may conceal protracted growth within the analytical uncertainty.

The question is still there whether or not measuring whole grains or even fragments of grains by ID-TIMS, which averages age by volume and U content in the grain/fragment, captures that last bit of crystallization within analytical resolution. One way to evaluate this is to look at young volcanic rocks where multiple dating methods can be used with absolute precision in the thousands to tens of thousands of year range. In nearly every such study, the youngest U-Pb date on zircon matches well with either Ar/Ar or He dates from the same deposits. But in any case, the problem of dispersed zircon populations in volcanic deposits is discussed in detail in many papers, so we needn't go on about that. The more important challenge in the redbole zircon data is the proposition that there is primary ash in them, which is discussed above and below.

One last note on "model their way out of this problem." It is very important to understand that all dates determined by geochronology involve a model at some level, whether that be a series of assumptions or actual statistical models to generate ages from dates. There are some good ones and some less good ones. The Bayesian eruption age model is based on the assumption that zircon didn't crystallize after eruption, which we know to be true. To be accurate, it requires that the zircons didn't experience Pb-loss, that we haven't biased our data by what zircons we pick, etc. In Schoene2019 we test the dependence of our results by trying many different models to see if the choice changes our proposed depositional ages and show it doesn't. One could also say that the Ar dataset is trying to model their way out of the problem of very low precision on single analyses by measuring multiple very large multigrain aliquots of plagioclase and taking weighted means. A weighted mean is a model which requires that the grains all record an identical moment in time, and that none experienced alteration, Ar loss, Ar recoil, etc. As higher precision Ar/Ar dates of sanidine have begun to be produced in the last few years, these new, disperse, datasets have shown that this assumption needs to be questioned in Ar/Ar geochronology. Thus, in both U-Pb and

Ar/Ar geochronology, dates are based on models that hinge on previous work and assumptions, and these should be continually tested.

R: The point here is that we have no basis to evaluate the assertion that these zircons were deposited in red boles directly from pyroclastic eruptions. Their ultimate source, at least for the ones closest in age to the lavas, must be volcanic but we have no assurance that they are not reworked. Distal silicic tephras are highly labile materials - they drape landscapes and are redistributed by wind, rain and gravity, on variable timescales.

A: Yes, this a description of a way in which it may be possible produce our data without primary ash in each redbole. See discussion above.

R: This leads to another issue. If we consider the interpreted Deccan eruption ages between sample BR and X (Poladpur Fm.) and between BH and O (Ambenali Fm.), within each of these intervals the interpreted eruption ages are all indistinguishable. The Bayesian constraint does what it is told to do and creates a positive accumulation rate. But we have no evidence that these indistinguishable zircon age populations aren't just reworked repetitions of essentially the same populations, and that therefore the steep volume/time slopes are fictive.

A: A response to this is covered above.

R: Pyroclastic eruptions energetic and voluminous enough to distribute tephra, including zircons, hundreds or thousands of km away generally produce calderas whose erosional remnants (i.e. granitoid plutons, ring dikes, etc.) are unmistakable. The closest candidates to the Western Ghats are in Gujarat, some 300-500 km northwest from the closest section of Schoene et al. (2019), but these (and their deposits, Sheikh et al, 2020) are very small and seem incapable of producing the kind of eruptions necessary to deposit tephras at such distance by direct airborne deposition. Yet, if all the zircon samples reported by Schoene et al. (2019) represent distinct eruptions, then we are talking about 24 eruptions of relatively large magnitude in âĹij 700 ka, i.e., a mean recurrence interval of âĹij30 ka. In contrast, large eruptions from a single eruptive center typically have recurrence intervals >50 ka (and often much greater, e.g. Yellowstone) which suggests (if we accept the primary deposition interpretation) that multiple large calderas or silicic vent complexes were involved, and are undiscovered.

A: It is clear we don't know where the sources of the zircons were. Our recent work in the Narmada Valley, Gujarat and the Malwa Plateau (Eddy et al., 2020; Basu et al., in press) don't answer this question either. But the combination of these studies does suggest the zircons wouldn't have come from a single eruptive center, so the argument above based on recurrence interval of a single eruptive center is not very relevant. Again, we now have geochemistry and Hf isotopic data from all the zircons from all of our Deccan work, which we will publish soon, and it will help answer this question. We should also keep in mind that most proximal possible locations for zircons in the Western Ghats is to the west, pretty close, and currently underwater. Regardless, if the sources being very far away is an issue, it is also an issue if you want to argue that they are detrital and transported uphill hundreds of km to the site of deposition without being rounded.

R: An alternative possibility is that many of the zircons are reworked, which renders the applicability of a Bayesian age model – no matter how elegant when applied appropriately- invalid a priori, and the apparent precision enhancement resulting from it spurious. Undoubtedly the authors are influenced by the apparent cohesion of U/Pb zircon dates from ashes interbedded with the CRB (Kasbohm and Schoene, 2018). But that is a very different situation, wherein there are known sources nearby and the identity of the ashes as primary pyroclastic deposits is unambiguous.

A: We note that it is true that there are known sources of volcanic ash in the CRB, that these are up to 250 km from some of the deposits, and a bunch of those dates come from redboles of similar character to those in the Deccan Traps (i.e., little indication in outcrop whether or not they are volcanic or have zircon). A difference is that 90% of redboles have zircon in the CRB, compared to ∼15% in the Deccan Traps.

R: An important implication of the zircon-based age model that cannot be ignored is that the interpreted peaks in eruption rates are followed by hiatuses. Since these inferred peaks are interpreted to characterize the Deccan Traps as a whole, these hiatuses (i.e. at the top of the Poladpur and Ambenali Fms.) would have produced regional disconformities. The one at the top of the Ambenali Fm. is required by the age model to be 300 ka in duration. Yet there is no evidence for an erosional disconformity above sample "O" in the Sinhagad Fort section, where such a disconformity would have to be manifest, nor in any other sections exposing the Ambenali/Mahabaleshwar fms contact that we have examined. 300 ka is a long time for a lava flow to be exposed at tropical latitudes without leaving a trace such as incision or a paleosol.

A: Dr. Renne is absolutely correct about this. For example, parts of Hawaii with flows 300 ka at the surface (NW side) are deeply incised 100 m or more. One may expect to see those in the Deccan if the hiatuses are real. One could also argue the overgrown outcrop and rugged topography would make this difficult to identify without mapping and chemostratigraphy at a level that hasn't yet been done. As discussed above, it would be remarkable if the Deccan shield volcano was composed of layer cake stratigraphy 360 degrees around a single or tightly spaced number of eruptive centers. If not, there would certainly be local hiatuses at least, and therefore disconformities. We don't know of any that have been described. But even if the scenario we explore above whereby the hiatuses are the result of local hiatuses and/or miscorrelated stratigraphy, it still requires hiatuses with no obvious physical evidence. It's perplexing.

R: Hence I strongly disagree with the statement "The apparent discrepancy . . . are beyond the scope of this paper . . ." (lines 156-159), as in fact this topic is central to the veracity of the U/Pb-based age model.

A: To reiterate, the reason we say it's beyond the scope of the paper is that we do not argue in this paper that the U-Pb based age model is necessarily correct. Instead, we say the Ar/Ar and U-Pb datasets agrees very well with each other, and that the difference in analytical precision prevents the Ar/Ar data from being able to test the variations in eruptive rate suggested by the U-Pb age model. And that Fig. 4 from Sprain2019 is misleading. We can now, however, use Fig. 4 from our paper to point out where they actually don't agree (if we choose to ignore systematic uncertainties) and move forward from there.

R: The discussion of the effects of different calibrations of the Ar/Ar system seems gratuitous and would probably confuse readers. Is the point being made here that the calibration of Kuiper et al., if correct, unambiguously shows that the zircons are entirely reworked and that their U/Pb ages are irrelevant to the lavas bracketing the boles in which they are found?

A: Uh, no, that's not what we're saying.

R: I personally enjoyed this discussion, which I think summarizes the current situation fairly and accurately, but a reader less steeped in the topic may be lost here. More importantly- and this goes back to what I said in the first paragraph of this review- there is a danger that readers may interpret the conflicts between the two age models as a measure of comparability between the two methods. This is clearly not the case when so many layers of assumption and interpretation are built into the U/Pb study.

A: The point being made is that there are systematic uncertainties that are still very large, and work still needs to be done on this. This makes it difficult to know if the apparent disagreement in ages in the upper Ambenali (Fig. 4) is real or if shifting both datasets relative to one another would just create differences in other spots.

R: Speaking of calibrations, some of the results in Schoene et al. (2019) are repeated from Schoene et al. (2015)- except that the ages have been changed, For example, sample "P" from the Sinhagad Fort section, which contributes to the inferred middle and most voluminous eruption rate pulse, is assigned an age of 65.883 in Schoene et al. (2019), but 65.651 Ma in Schoene et al. (2015). The younger age is completely consistent with Sprain et al. (2019), but the latter is not. These two summary ages are based on the exact same data set. In detail, the most precise single zircon 6/8 age (sample z4) is stated as 65.65 ±0.13 Ma in 2015 but 65.75 ±0.28 Ma in 2019. Note that this most precise age (in the 2019 version) is resolvably younger than the age assigned to its population- especially when systematic uncertainties are excluded. How is this justified? The change from 2015 to 2019 appears to be mainly the result of a different correction for Th/U initial disequilibrium, but this is only implicitly explained in the Supplementary Materials of Schoene et al. (2019), which states that the maximum change due to the updated correction basis is 0.04 Ma. This is not to criticize the authors for using a correction that they feel is most accurate (and more conservative), but rather that discussion of the effects of different calibrations and corrections should probably include ones that have 100's of ka effects, in some cases well beyond stated uncertainties.

A: The shift in dates of RBP noted by Dr. Renne is not a result of Th/U disequilibrium, which as he knows can only affect dates by ~0-30 kyr for reasonable choices of Th/U of the magma. This was explored in Table S3 of Schoene et al. (2015). The explanation for RBP is as follows: RBP is unique in our dataset in that the redbole has abundant very small and low U zircons. Hence, they have very low ratios of radiogenic-to-common lead (blank), which is the primary control on the precision of a date. It also means that its zircon analyses are much more susceptible to the choice of isotopic composition of the blank, and the associated uncertainty in that composition. The blank composition is used to estimate how much blank 206Pb and 207Pb to subtract from the total 206Pb and 207Pb, leaving the radiogenic Pb used to calculate a date. So if that subtraction is large and uncertain then the date is uncertain. Between 2015 and 2019 we did a much more comprehensive set of measurements trying to characterize this blank composition, which resulted in a 206Pb/204Pb blank value with much larger uncertainties. So each zircon date from Schoene2015 that was reproduced in Schoene2019 was older and less precise. Excluding RBP, the average shift was 17 kyr older and the average increase in uncertainty was 18 kyr. For RBP, the average shift was 226 kyr older and the average increase in uncertainty was 250 kyr. Because this sample is so uncertain with respect to the others, it has little effect on the overall age model.

Regardless some of the preferred final depositional ages shifted by 50-70 kyr older, which is significant. These are primarily the dates from the Mahabaleshwar formation. This is a result of both the approach used for age determination (geochemically guided low-N weighted means in 2015, versus Bayesian eruption age calculator in 2019) and the addition of new zircons and samples used in the Bayesian stratigraphic age model in 2019. In particular the upper samples in the Mahabaleshwar shifted older by tens of kyr. This is actually mostly due to the weight put on sample RBE, which contained three younger zircons from the MIT dataset (maybe a systematic difference between labs?). The stratigraphic model used in Schoene2015 based on a naïve sampler required those dates to be honored more strictly than the current, better, Bayesian model based on a Metropolis sampler. The result is that the final ages for those samples (DEC13-08, -09, -10) were pulled younger than with model used in Schoene2019. We have a spreadsheet that summarizes all these changes that we'd be happy to send to anyone who is interested.

R: In summary, I think that a paper such as this could be useful if it realistically depicts the geologic factors at play- not just the intrinsic differences between radiosotopic systems.

A: Again, this is not a paper about intrinsic differences between dating methods, it is about how to plot and interpret data accurately.

R: Ultimately, I wonder whether the present authors are the right people to write it. It is undoubtedly very difficult for them to be as self-critical as is required to make this a useful contribution. I don't mean this as a personal attack- I have high regard for the authors- but just to say that it is inherently difficult for them to be objective about their previously published work, as it probably would be for anyone.

A: . . .

R: Paul Renne

Interactive comment on Geochronology Discuss., https://doi.org/10.5194/gchron-2020-11, 2020.