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

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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.

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.

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.

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. Worse yet, to apply this assumption to redefine a formational contact (Poladpur-Ambenali) by $\sim$100 m, as done by Schoene et al. (2019) is clearly circular. 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. 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.

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.

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.

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. 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.

More fundamentally, interpreting highly dispersed (relative to analytical precision) zir-

con 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.

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.

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.

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 $\sim$ 700 ka, i.e., a mean recurrence interval of $\sim$30 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.

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.

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. 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.

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? 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.

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 $\pm$0.13 Ma in 2015 but 65.75 $\pm$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.

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. 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.

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