This work by Liesa Brosens and colleagues takes a very interesting approach to lavaka analysis in Madagascar, and I applaud their broad thinking.

However, I share the concerns about model accuracy and reliability raised by Ben Purinton in his review, and wanted to make a few comments on those issues, in addition to raising some additional questions about the geomorphology. These points are intended in the constructive and collegial spirit of the open review process.

The bulk of the image data on which the authors' model is based is not high resolution: only two of their study areas are imaged at 20 cm/pixel, all the others are imaged at 12-30 m/pixel. Many lavakas are only a few pixels across, and elevation changes are in many cases close to the resolution of the imagery, which will not capture internal relief changes within the gullies. It also means that there must be substantial potential error with the area calculation, as a lot of edge detail will not be resolved; and lavaka edges can be quite complex in shape.

The authors apply a simple least-squares fit to area-volume data to provide a relationship that they then use to drive much of their analysis. I did not find a graph of the XY data that they used, or an R^2 value or other measure of quality of fit. But I expect that those data are very noisy, and the relationship is not very precise. To test this, I looked at the data that the authors made available on Fileshare, and I plotted area vs relief (as a proxy for the depth data, which was not in the file). I will share two observations. First, the linear fit fails at smaller volumes. This is a problem, because lavaka size follows a power-law distribution, with the majority being at smaller sizes: a robust line of fit should model the bulk of the population, so I think the authors should consider whether it works to have a large proportion of the population not represented by the fit line. Second, the data clouds for the different study areas have different lines of fit, so a one-size-fits-all approach may not be valid.

I also worry about the circularity of using area to derive volume, and then using that derived quantity to an area-volume model, as the authors do in Fig. 5. I would want to see a much more detailed unpacking of the caveats that attend this approach, and in particular I think it would be important to show the original area-volume data used to derive the base relationship. This method may provide interesting ways to look at and think about landscape evolution at a broad scale, but I do think that the noisiness of the base data, and the imperfection of the original line of fit to those data, warrant large error envelopes; and severely limit the precision of downstream models based on that original line of fit. The authors should go into these issues in more detail, because it feels as though they are somewhat brushed under the rug (see comment in previous paragraph about no figure showing the original data relationship with its fit line). It seems to me that there are multiple nested and cumulative uncertainties, which could be more completely and thoroughly addressed/ (I hope I am not missing something obvious here, and will be happy to be corrected if I am).

I am concerned also about the underlying geomorphology. Lavakas evolve through different stages, with very different activity levels over time (per the excellent and detailed work of Neil Wells and Benjamin Andriamihaja). Older lavakas are larger, but also evolve to be more shallow

than (see Wells et al., (1991) ESPL 16: 189-206, Fig. 3). Each lavaka follows its own adventure in terms of growth, deepening, and shallowing (with the shallowing due in large measure to capture within the lavaka of the erosional materials from its walls). So I have two points here. First is that the known geomorphology of lavakas should probably be considered in any model for their evolution through time; and second, that I would like to see more justification for using landslides as analogues (because although lavakas have headscarps, and evolve via collapse processes, they do not really behave like landslides because of their narrow outfall channels). This may mean that some of the assumptions regarding bias-correction factors may need some adjustment, as these which come out of lanslide modelling (per Lines 195-200), and are baked into the authors' model for area-volume relationships. I'm not saying that landslides are an inappropriate analogue; but I am saying that the authors should provide a firm geomorphologic rationale (which I cannot find section 2.4 or elsewhere in the current manuscript).

In calculating sediment mobilisation rates, the authors state (Line 210) that they used a bulk density of 1.5 t/m^3, based on soil corings 2 m deep. This value is likely to be too high. The surface laterite layer in this area is a couple of m thick, so this is what the cores will have sampled. Below this, and forming the bulk of the material that is evacuated from lavakas, is saprolite, which is highly porous and therefore has lower bulk density. A better value would be 1.1-1.2 t/m^3, in line with previous work (e.g. Heimsath et al. (1997) Nature 388: 358–361, Montgomery (2007) PNAS 104: 13268–13272, and the 2008 UVM thesis of Matt Jungers).

Finally, I query the authors' conclusion "current mobilization rates exceed the long term rates by two orders of magnitude" (Line 370). The problem is that the comparison being made is between apples and pears: although the authors do provide the proviso that "not all mobilised lavaka sediment will end up in the rivers", they are assuming that these very different datasets are comparable. In fact, most of the (limited, for sure) evidence suggests that a lot of lavaka sediment is deposited close to (and even within) the lavakas themselves (similar to the landslides on which they model lavakas). Thus, long-term lake infill and river-sediment-derived erosion estimates will miss this material. If lavaka sediment didn't make it into those archives in the past, then the values from those archives cannot be used as a comparison with mobilisation rates from modern lavakas.

I admire the amount of work and data collection that went into this manuscript, and I hope that the authors will accept this critique in the collegial spirit in which I offer it.