Response to Reviewer's Comments   10 Sept. 2024

Reviewer A:
Comments:
The manuscript provides a new and comprehensive overview and analysis of seismicity before , during and after the Harrat Lunayyir dike intrusion in 2009. The work describes the seismicity to understand magma flow pathways and uses mainly earthquake magnitude to model magma volumes. The work is novel, well written and illustrated, and very much a valuable contribution to the literature. I have a few fairly minor comments that I hope help to improve the manuscript.
Reply: We are pleased to receive the reviewer's positive comments, particularly that the reviewers believes this manuscript is a "valuable contribution" and the necessary revisions are "fairly minor".

Pg 2 line 37 The phrase "While much can—and has—been said about this tectonic seismicity, it is not our focus and is mentioned for the sake of completeness; we instead focus our attention on seismicity with an apparent volcanic origin." comes a little bit out of the blue and comes across as very awkward. Why not just replace with a simpler sentence such as "Deep earthquakes also occur in volcanic settings, and it is the origin of these that is the focus of our paper."
Reply: Done as recommended.

Fig 1 –please make the lat and long tick marks, or marker grid clearer. This applies to all the figures. Perhaps tick marks down the map sides would be clearer than the very pale grids shown now.
Reply: Done.

Pg 2 line 115 – it would be helpful to have a few extra words regarding how the SGS go about locating the earthquakes e.g. velocity model , minimum number of phases , code used for the locations etc etc.
Reply: Done.

Line 207 - I got confused when I read "1/20–2". Perhaps simpler to state 0.05 to 2?
Reply: Done.

Pg. 7 generally - Also, more generally in this section I do appreciate that the idea is to find a simple way to estimate intrusion volume from seismicity, but the assumption that the high frequency double couple earthquakes are directly from dike opening is somewhat flawed. Tensile cracking is generally thought of as being related to lower frequency and non-double couple earthquakes. In my opinion more discussion justifying the approach taken and assumptions made would be appropriate.
Reply: Recommendation accepted. Intrusive volume is a secondary topic in this paper. Hence, seciotn 2.3 on "seismic constraints of magma volumes" has been deleted in view of the uncertainties in the calculation.

Section 3 lines 230 – 236 – this first paragraph would fit better in the data and methods section.
Reply: Done.

Lines 240 – a few additional words about the relocation procedure would be appropriate here.
Reply: Done.
Fig 7 – the panels are incredibly small and difficult to see anything in them at all. Perhaps it would be clearer to split into larger time intervals so the number of panels can be reduced. Also the thick purple stripe (regional border?) just adds unnecessary complexity to the panels, along with all the other similar style maps in the paper – can it be removed? Could also consider lightening the background satellite image so the earthquake dots are easier to see.
Reply. Agree with comment We have deleted the Figure7. Figures 6 and 8 provide the same information, only more clearly.

Recommendation: Revisions Required
Reply: Thank you for the helpful review.

Reviewer B:
Comments:
This manuscript presents an overview of a seismic catalog recorded over 24 years in the Harrat Lunayyir volcanic field. The authors detail seismogenic features that are convincingly demonstrated to be related to ongoing (subsurface) volcanic activity. They perform an analysis to relate observed seismic moments to magma volumes, and use their model to estimate the volume of magma intruded into the mantle following a 2009 near-surface intrusion. The attempt to relate seismological and volcanological quantities is novel and interesting, and may be deserving of publication in Seismica.
Reply: These positive comments are encouraging.

The paper is adequately written to communicate its subject matter, although I do recommend some changes to style below. More seriously, I identify three major methodological issues with the authors' approach that must be addressed before the manuscript may be considered suitable for publication; the most serious issue arises from the proposed technique to relate seismic moment to the volume of intruding magma.
Reply: (1) We accept the recommended changes to style. (2) The three major methodological issues mentioned by the reviewer are discussed one-by-one below.

Major comments:

Magnitude scaling
To retrieve moment magnitudes for seismicity in the SGS catalog, the authors assume that local magnitude is equal to moment magnitude. Without knowing more about the methodology by which local magnitudes are calculated, it is nontrivial to assess whether this is an appropriate choice; indeed, this assumption may introduce significant error into the estimation of Mw. The widely adapted magnitude scaling relation of Hanks and Kanamori (1979), for instance, prescribes log M0 = 1.5 ML + c, a very different slope from the value of 1 used here; just as significantly, many studies have shown that the relationship between Mw and ML may become nonlinear for small (M<2) earthquakes, with ML values possibly being underestimated by as much as one magnitude unit (relative to Mw) for these small earthquakes as a result (Deichmann 2006, Ross et al. 2016).

The choice of how to retrieve M0 values from local magnitudes is an important one for the analyses presented in the manuscript, which attempt to quantify magma intrusion volumes from seismic moment**.** Although it will not be possible for the authors to calculate moment magnitudes from waveforms directly, as they mention they have no access to the raw waveform data for most of the catalog, I would strongly recommend that the authors undertake additional analysis to demonstrate that either (1) the Mw = ML approximation is sufficient for their catalog, or (2) their results, including estimations of magma intrusion volumes in the mantle, are not significantly affected by the choice of magnitude scaling relation.
Reply: We no longer need use moment magnitudes since we have deleted the section on the calculation of magmatic intrusion volumes from the moment magnitudes.

Tracking vertical propagation of magmatic fluids
The authors present several arguments that their catalog evidences episodes of vertical migration of seismicity, and hence vertical propagation of magmatic fluids. A week-long vertical migration at crustal depths of 25 to 15 km between 29 April 2009 and 3 May 2009 is strongly supported by their Figure 6b. However, I believe the evidence they provide for other episodes of vertical migration is tenuous.
Reply: We agree with the reviewer, and we have removed statements of the other episodes of vertical migration. The discussion of the week-long vertical migration from a depth of 25 to 15 km has been retained, as recommended.

On line 272, the authors describe pre-2009 patterns of seismicity as "the clearly episodic and vertically migrating pulses of seismicity in 2008 and earlier," referencing Fig. 2. I see no evidence for vertical migration of pre-crisis seismicity in this plot or other plots; if vertical migration is present, it should be illustrated by plotting closer looks at these episodic swarms. Similarly, the authors describe one post-crisis sequence of seismicity in the lower crust on line 365: "We note the sequence in 2014 follows a pulse of mantle seismicity in 2013, delayed by a few months and is followed by a pulse of seismicity at <20 km depth near the deepest extent of the dike (delayed by a couple of weeks (Fig. 9b)." While the sequences do occur as described, mantle seismicity and <20 km depth seismicity are continuously active in the years before and after the 2014 episode. The continuous activity makes it unclear whether the lower-crustal seismicity of 2014 is genuinely related to the deeper and shallower seismicity.
I also find the evidence for migration within post-crisis sequences unconvincing (Fig. 13). The migration within sequence 1 is described as (line 347) "an initial upward vertical migration at ~5 km/day for ~12 hours (2.5 km traversed vertically)". The fit of the migration line (Fig. 13a) to the data is unsatisfactory and dependent on a small number of earthquakes. Additionally, the authors estimate depth uncertainty for these events on the order of 1 km (line 345); because the depth uncertainty is on the order of the 2.5 km vertical extent of the seismicity, a reliable assessment of vertical migration cannot be reliably made from these data. The purported vertical migration of sequence 8 (Fig. 13b) suffers from similar issues. The migration velocity is reported as 35 km/day in the text (line 353) and 32 km/day in Fig. 13b. Like sequence 1, it is unclear from the small number of earthquakes whether the 32 km/day migration line meaningfully fits the data, especially given depth uncertainty.
Reply: We agree. These claims have been omitted and we have dropped Figure 13.

The authors conclude (line 504) that “Vertical propagation of magmatic fluids is trackable from seismicity.” Vertical migration of seismicity is evident during the 2009 sequence, but it has not been satisfactorily illustrated that the same behavior accompanies the pre- and post-crisis seismicity. I would suggest the authors concentrate their analysis of vertically migrating seismicity, and the potential for identifying propagating fluids, on the sequence illustrated in Figure 6b.
Reply. We agree. Done.

Estimation of magma volumes from seismic moment
The authors present a conceptually simple framework for relating seismic moment to volume change. They propose the use of the form M0 = AΔV, where A is a scaling parameter and ΔV is volume change (equation 5, line 195), citing the work of Muller (2001). However, this is a misrepresentation of the relation proposed by Muller (2001), which specifically relates the *isotropic* moment of an earthquake to ΔV, rather than the *entire* moment: MISO = AΔV. The authors of this manuscript instead use the entire moment (isotropic + deviatoric) to estimate A, and then use their estimate of A to estimate the volume of magma intrusions. This is equivalent to an implicit assumption that the mechanisms of all earthquakes can be described as completely isotropic, with no CLVD or double-couple components. While I am cognizant of the fact that the authors present their volume results as simple order-of-magnitude estimates, this is a crucial, unaddressed point that leads to further, compounding order-of-magnitude uncertainties in their model. There are no moment tensor solutions discussed in this work, so the actual value of MISO for any given earthquake is completely unconstrained. The authors explicitly state the assumption that HL earthquakes are due to melt migration in a tensile crack, but the assumption of a 100% isotropic mechanism for any earthquake, even in this setting, is nonphysical; even a pure endmember crack-opening mechanism does not result in an earthquake with 100% isotropic moment (e.g., Vavrycuk 2001). Therefore, the M0 value of any of these earthquakes will be an insufficient proxy for ΔV.
Additionally, the manner in which A is calculated requires further explanation. The authors estimate a value of A ~ 1 GPa using equation 5. They use a value of ΔV from geodetic estimates of the volume of the near-surface dike which intruded in 2009, and a value of M0 taken from a single Mw 5.4 earthquake which occurred during the intrusion. It is not clear that the choice to use the M0 of a single earthquake in this equation is physically motivated; the authors state that the 2009 dike was “commensurate with the largest earthquake,” (line 204), but this alone does not explain why this single earthquake should account for 100% of the seismic moment release associated with the diking. The authors also calculate a different value of A ~ 4 GPa using a “larger M0” (line 205), but they do not explain how this larger M0 value was calculated or why they prefer the A ~ 1 GPa value.
The issues outlined here are difficult to surmount without a method to properly estimate MISO. I suggest the authors repeat their magma-volume analyses using the alternative relation they propose from White and McCausland 2019, which is empirically founded, would allow the authors to avoid many of the issues described here, and has already been shown to be able to reasonably estimate the volume of the 2009 intrusion (line 207).
Reply: We agree with the reviewer that our estimations of magma volumes are uncertain and we have omitted this section of the paper.

Minor comments:

Line 29: "Non-volcanic earthquakes within the continental lower-crust and lithospheric-mantle are rare, but of great scientific interest. These are attributed to tectonic forces and phase-changes within the lithosphere." The second sentence should be cited.
Done.
Line 30: The authors discuss controversy around the occurrence of lower crust and mantle seismicity (I assume they are referring to stress considerations) without explicitly stating the nature of the controversy. Please include a (brief) explanation of why deep seismicity is considered controversial.
Done.
Line 51: "Often [stage one volcanic seismicity] goes unobserved, due to limitations of local seismographic networks, though it has been confirmed through careful reexamination of seismic records guided by petrologic insight." Please clarify what this means; as written, the sentence suggests this activity is universally "confirmed" wherever volcanic activity occurs. Where in particular has it been confirmed? Please cite examples.
Reply: Omitted "though it has been confirmed through careful reexamination of seismic records guided by petrologic insight."
Figure 1: HL should be labeled on the map.
Done.
Line 79: the authors make reference to the "2009 crisis" throughout the paper, but it is unclear on initial reading why the activity warrants this label (which implies hazard in a populated area). Later in the paper (line 273) an evacuation of the local population is mentioned; this should be discussed earlier if the term "crisis" is to be used.
Done.
Line 111: please provide the number of events in the catalog (it is given later in the paper on line 143, but it should be mentioned here as well).
Done.
Line 128: "BGMM, as implemented in the scikit-learn python library, to separate the better located post-crisis catalog into five categories of seismicity…" This is a sentence fragment.
Fixed.
Line 159: "One possible hypothesis to explain the crustal seismicity at HL is that the dike intrusion was essentially a slow extensional earthquake and that the earthquakes in the upper crust since then are merely aftershocks of this episode, similar to that observed on Kilauea (Dvorak et al., 1994)." This reference is irrelevant to the analysis you are proposing. The Dvorak paper is concerned with the aftershocks of a M7.2 mainshock, not one particular dike intrusion.
Agreed.
Line 165: "Seismic Sequences." The distinction between "aftershocks" and "seismic sequences" is linguistically imprecise; aftershocks also form seismic sequences.
Agreed.
Line 207: Please replace "1/20" with the more readable "0.05".
Done.
Line 258: "[the 2000-2008 seismicity is] dominantly located in a small (~30 km diameter) cluster just SW of the southernmost portion of the 2009 dike intrusion (Fig. 5)." The seismicity in Fig. 5 appears to be to the southeast of the dike, not the southwest.
Agreed. Fixed.

Line 258: "The seismic activity occurred along vertical pipe-like trends that look suspiciously like they track fluids (magma) ascending vertically…" From Figure 5 alone, which includes a time-depth plot, the authors have not demonstrated the geometry of seismicity, only that it extends from ~2 to 40 km depth. Please plot a depth section of seismicity to illustrate these pipe-like trends. Please also cite previous work which has related vertical pipe-like seismicity to fluid ascent.

Done.

Figure 7: Stars appear on some plots (e.g. e3) without labels. If these have the same meaning as in Fig. 6, please label them.

Done. Figure caption says "Stars are earthquakes with magnitudes >4."

Line 320: "Magnitude of completion" should be "magnitude of completeness."

Done.

Line 324: "The mantle earthquakes detected beneath HL from July 2010 onwords occur 40-50 km below the surface." This is a sentence fragment, and "onwards" is misspelled.

Fixed.

Figure 10 caption: Please specify that these sequence detection results are for post-crisis seismicity, to better distinguish this figure from Fig. 5b.

Done.

Line 336: Please parenthesize "Table 2".

Done.

Line 349: Typos: "We also to looked at this sequence… no knew insights".

Done.

Line 394: "The initial seismicity after the 2009 dike intrusion, through July 2010, can be modelled as an aftershock sequence." Please specify here, as you do in the Fig. 15 caption, that the Omori fit is only to 0-15 km depth seismicity.

Done.

Figure 15: The date labels on the x-axis are illegible; please reformat the tick labels.

They are legible.

Line 414: Typo: "tmperature"

Fixed.

Line 428: "We therefore assume that within the lower lithosphere only ~1% of the moment (and perhaps only 0.1%) is released seismically…" The inclusion of the "0.1%" figure may confuse the reader, because the authors' following analysis relies only on the 1% figure. Please clarify, or remove the 0.1% parenthetical.

Fixed.

Line 450: "It is clearly still being driven by magmatic fluids…" While seismic activity is continuing, this phrasing is ambiguous and may also be interpreted as meaning that the dike is continuing to intrude further into the crust; please clarify.

Fixed.

Line 486: "Indeed, the post-crisis (post-2010) mantle seismicity has a total moment (> 10^14 N m) now approaching the deep pre-crisis deep seismicity total moment (< 10^15 N m) that some might argue could have been used to forecast the HL near-eruption." It is unclear that a particular value of total moment could have been used to forecast the 2009 intrusion at HL. Please include a citation, or expand on this thought to provide additional reasoning.

Omitted. Too speculative.

Line 495: “Mantle seismicity is driven by magmatic fluids, possibly volatiles but likely melt…” As a concluding point, this sentence does not adequately summarize hypotheses already put forward in the paper; the authors do not provide a justification for why they prefer melt as an explanation over volatiles elsewhere in the paper.
Agreed. Fixed.
Line 499: “Much of the shallow seismicity in HL can be considered as aftershocks of the 2009 dike intrusion…” Please specify 0-15 km depth, rather than shallow. The 15-25 km depth seismicity emphatically does not follow Omori decay, as shown by Fig. 15.
Done.
Table 3: The assumed melting temperature 1372°C is very specific. If this value is informed by other work, or was chosen for a particular reason, please explain further in the text.
Throughout the paper, “lower-crust” and occasionally “lithospheric-mantle” are hyphenated. I recommend using the more commonly accepted non-hyphenated forms: “lower crust” and “lithospheric mantle”.
Done.

References

Deichmann (2006). Local Magnitude, a Moment Revisited. Bulletin of the Seismological Society of America https://doi.org/10.1785/0120050115
added
Hanks and Kanamori (1979). A moment magnitude scale. JGR: Solid Earth https://doi.org/10.1029/JB084iB05p02348
(already in references)
Ross et al. (2016). Analysis of earthquake body wave spectra for potency and magnitude values: implications for magnitude scaling relations. Geophysical Journal International https://doi.org/10.1093/gji/ggw327
added
Vavrycuk (2001). Inversion for parameters of tensile earthquakes. JGR: Solid Earth https://doi.org/10.1029/2001JB000372
Not needed.

US Geological Survey Peer Reviews and Replies

USGS internal review of Blanchette et al
David Shelly, USGS

This manuscript presents an impressive synthesis of seismicity near Harrat Lunayyir from 1998-2022, which includes fascinating depth variations in seismicity from the mantle to near the surface. The authors examine various features of this seismicity, classifying it into different zones and examining its spatial and temporal evolution as well as the distribution of magnitudes (and b-values) along with seismic moment. These features are then related back to physical processes. The authors discuss numerous challenges with this large dataset, including heterogeneous seismic coverage, limited parameters available in the seismic catalog, and the general challenges of recording deep seismicity. Overall, this manuscript presents a comprehensive analysis. It is mostly in good shape, but I think that it could benefit from some minor revisions.

Specific comments:

The introduction, section 1.1 needs to be rewritten for greater clarity.

Done.

I found the plotted seismicity difficult to see in Figure 7, with the background satellite imagery. I would suggest lightening this imagery when it's deemed important.

Figure 7 was of poor quality, not essential, and has been removed.

Lines 128-130: Currently a sentence fragment (should it start with "We applied…" or something like that?)

Fixed.

Line 380 (and a couple of other spots) - I wouldn't lean too heavily on the Wiemer and McNutt study as far as fluids implying high b-value. They only looked at sequences from two volcanoes. b-values can be influenced by a variety of factors.

Understood. However, the Wiemer and McNutt (1997) study is a classic reference.

Line 414: typo: temperature

Section omitted based on journal reviews, so this word has also disappeared.

Jeanne Hardebeck, USGS Review and Replies

Line 114: This says that 4 or more stations are required for location, but in Figure 2b it looks like there are only 2 or 3 stations installed during 1998-2008, and line 250 says there are only 3 stations within 100 km. Please clarify.

Agreed. We have restated this as "All post-2009 events were located using arrival times at three or more seismic station."

Line 118: "other portions" -> "older portions"?

Fixed.

Line 121: How well are the depths constrained for the oldest events that are recorded on only ~3 stations? With (presumably) large depth uncertainty, can you reliably categorize these events by depth?

Agreed. Pre-2009 seismic depths are less well constrained for the oldest events. However, the main scientific conclusions presented here are based data from more than ten seismic stations (i.e., over 100 sub-Moho earthquakes displayed in Fig. 2b). By 2012 the number of seismic stations was more than 20. This information is clearly presented to the reader in Figure 2b which displays the seismic station density versus time.

Line 155, Equation 3: Please respond to reviewer B's comment that local and moment magnitude are not necessarily equal.

Since we have deleted the attempt to use the moment magnitude to calculate the magma volume for the main intrusion, the accuracy of the conversion from local to moment magnitude is not critical to any scientific conclusion presented in this paper.

Line 197: Please respond to reviewer B's comment that Figure 5a can't demonstrate pipe-like structures, because it only shows the depth extent of the seismic activity and not its spatial width. A couple of cross-sections would be required to show a pipe-like structure. (With a depth extent of ~30 km, and a ~30 km diameter (line 196), this does not sound very pipe-like.)
Agreed. Figure 5a only shows the remarkable depth extent (about 40 - 2 km in depth) of the seismic activity. We have dropped all references to a pipe-like structure, which we haven't demonstrated.

Line 223: The area of the future mantle earthquakes is the red ellipse. These events occur in the blue ellipse, the location of future lower-crustal earthquakes.
Agreed.

Line 233: "orthern roups" -> "northern groups"?
Fixed

Line 277: Please respond to reviewer B's comment that this possible vertical migration is not well-resolved. The response to reviewers indicates that this claim should have been removed in revision.
Agreed. The claim of there being evidence for vertical migration has been removed.
Line 337: Define "A" and "V". "A" appears earlier in Omori's law, so if this is meant to be a different parameter, please choose a different symbol.
Equation omitted a not needed.

Line 401: "may increased awareness" -> "may have increased awareness"
Fixed.

Please include the disclaimer "Any use of trade, firm, or product names is for descriptive purposes only and does not imply endorsement by the U.S. Government."
Done.