Peer Review File

Manuscript Title: Remote Detection of a Lunar Granitic Batholith at Compton-Belkovich Reviewer

Redactions – unpublished data

Parts of this Peer Review File have been redacted as indicated to maintain the confidentiality of unpublished data.

Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referee #1:

Review of Remote Detection of a Lunar Granitic Batholith at Compton-Belkovich by Siegler et al.

I find this paper to be a compelling and exciting study where new results from the Chang'E-1 and Chang'E-2 Microwave RadioMeter (MRM) are presented for the Compton-Belkovich high thorium feature. In particular, evidence is presented to show there is likely a granite-like feature beneath the Compton-Belkovich feature. If true, this is a surprising result as granitic systems are rare beyond the Earth, and finding such a system on the Moon outside of the high-thorium Procellarum KREEP terrane would require some rethinking of how such processes could take place on the Moon.

While some of the details in this paper are beyond my area of expertise, in my reading of the paper I consider it to be generally sound, and should be published. I do have a number of comments/suggestions that should be considered by authors prior to any possible publication. Some comments are minor, and others are likely more substantive. My comments are given below in sequential order as they come up in the manuscript.

1. Lines 56 – 58: Maybe reword to say: "… are generally coincident with high Th concentrations detected with orbital gamma-ray data [4,5]". The original wording is a bit odd.

2. Line 59: The Lawrence et al. (2007) citation is not really the best one for discussing the original detection of the C-B anomaly. Rather, it is better to use Lawrence et al., GRL, 26 (#17), 2681, 1999 and Lawrence et al., JGR, 108 (#E9), 10.1029/2003JE002050, 2003. The Lawrence et al. (1999) paper provided the "discovery" result for the C-B anomaly, and Lawrence et al., (2003) provided the first estimate of the surface Th abundances of 40 – 55 ppm. This abundance estimate was later updated by Wilson et al., (2015) to be in the range of 14 – 26 ppm when using more realistic assumptions for a spatial reconstruction process. When quoting any orbit estimated Th abundances, therefore the best value to use is this value from Wilson et al. (2015).

3. Line 66: "… anomalously hot Apollo 15 site". Can a citation be provided for this number?

4. Lines 67-68: " … much higher radiogenic element concentrations than previously observed from orbit." In reading later portions of the paper (Section 5 of the Methods section), it seems that this may be referring to the 60 – 132 ppm abundances mentioned there. It may be useful to be reminded that the orbit-measured data are limited by the fact that the spatial resolution is relatively broad, and there could be small regions on the Moon with higher abundances than actually measured. Such features though would need to be relatively small (such as what was assumed in Lawrence et al., (2003)).

5. In regards to Figure 1 (and other associated figures in the Methods section). This is a single, relatively small area on the Moon, and I am curious what similar data look like elsewhere. Is this location really as strange as suggested in the text? For example, earlier it is suggested that Apollo 15 has a measurable heat flow. Is this backed up with the data? Part of the reason that I ask is that in an analogous fashion, the C-B thorium enhancement would not be that remarkable if it were located in the PKT. However, the primary reason it really stands out is that it is a thorium enhancement in an otherwise very low-thorium portion of the moon. It is possible that this is a similar situation for the brightness temperature and thus heat flow? This is one area for which I am not an expert (I do not have a deep familiarity with the MRM data). So, if this is a naïve question, I defer to the experts. However, it does seem a question that a general scientist might ask when reading the paper.

6. Lines 75, 78: The 'MRM' acronym is not defined in line 78, but could be conveniently defined in line 75.

7. Lines 96-102: When mentioning greater and shallower depths, it would be good to provide the notional depths that are represented here.

8. Line 110: Is the word 'shallows' correct? Should it be 'shows'?

9. Lines 139-148:

As I mentioned earlier, I would not use the ~49 ppm Th concentration (and the reference of Lawrence et al. (2007) is not the correct reference for that value; rather it is Lawrence et al., (2003)), but rather the 14 – 26 ppm from Wilson et al., (2015). Of course, there could be localized (smaller than the GRS spatial footprint) values that are much larger, so there is some leeway here.

Second, I'm not sure I fully follow the reasoning here about the heat flux. In this paragraph, it is mentioned there is a heat flux of 150 mWm-2, whereas in the 'Methods' section (line 665), the heat flux is given as ~180 mWm-2; I also note that in the abstract, there is a value of ~184 mWm-2 that is given. One further item that is not clear is how these values are arrived at. On line 81 and Figure 3a, there is a ΔT brightness temperature of ~9K; but when I then go to Figure 2c (the 'calibration' from brightness temperature to heat flux), I only get a heat flux of ~50 mWm-2. Maybe the larger number is arrived at when accounting for the broad spatial resolution of the microwave measurements? If this is the case, then it is not immediately clear to me how you got this value. So, if this explanation can be more clearly stated, that would be useful.

Third, I do not follow the reasoning of how the measured thorium concentration is related to the thorium concentration in the putative column beneath the surface. It seems that the subsurface thorium is being scaled based only on the heat flux, and the surface expression of this thorium does not necessarily have to have the same value. Is there a good idea for why the surface concentration of the thorium would be much lower than the concentrations at depth? Maybe there is some dilution of the intrinsic subsurface thorium concentrations with lower values during the pyroclastic eruption that put the thorium on the surface? Is such an idea worth exploring here in the paper?

10. Lines 161-169: I want to poke a bit at the assumption that there should be two bodies beneath C-B. While it may make geologic sense to have two bodies (line 166), is there anything in the data that requires or explicitly points to such an idea? I ask because it naively seems simpler to just have one "average" body if that is all the data can constrain.

11. Figure 4 seems to be missing a 'c)' label (at least in my copy).

12. Methods section, line 468: " … appears justified …". If I understand the text correctly, this is claiming that in Extended Data Figure 1, the red data points are well fit by the blue line, which is the model. However, two of the four data points do not fit the model within the error bars, and the other two points fit the model, but through large error bars. In fact, one might argue that a horizontal line would provide a better fit to the red data points (by eye, I can see that a horizontal line can go through the error bars of all four data points). Thus, is the claim "appears justified" really justified? Or am I misunderstanding what is being said?

13. Methods section, line 520: What is the nature of the latitude correction? This correction is mentioned a few times in the paper, and it is not clear what is being done for the correction.

14. Methods section, line 556: "… at all local time …": 'time' needs an 's' to make it 'times'.

15. Methods section, Extended Data Figure 3: In my copy, this figure is missing the a), b), and c) labels.

16. Extended data figure 4: This figure seems to be missing? (maybe it is the figure around line 630?)

17. Extended data figure 7: This figure appears to missing the labels?

18. Methods section, line 663: "… about 7 ppm from the pixon reconstruction …". You might want to note that the actual inferred thorium abundances at the surface (according to Wilson et al., (2015)) is 14 – 26 ppm.

19. Methods section, lines 694 – 697: "The presence of a more evolved upper body suggests …". As I suggested in a comment earlier (and will ask again here), is the existence of two bodies really proven or shown by the data, or is this conclusion speculative? Or to take in another way – would the data fit a single body or is there a data driven way to distinguish between the two scenarios?

David Lawrence

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Referee #2:

Dear Authors,

Congratulations on your completion of a very interesting and engaging manuscript. Given the recent interest in silicic volcanism on the Moon within the lunar community this research will add a significant and valued piece of the puzzle that is silicic volcanism. Little is known about the underlying magma chambers that feed silicic domes and your work will help to fill that knowledge gap. I do, however, strongly urge you to consider my comments about the chosen Th values for your model, discussed in detail below. I look forward to seeing the finished manuscript and discussing your work with you at future conferences.

Regards,
Dr. Sarah Valencia

Lines 53-54. Seddio et al. 2015 looked at the petrogenesis of intrusive vs extrusive granitic rocks by looking at the textures and Raman spectra of silica phases, not at the concentrations of radiogenic elements. I don't personally know of a mechanism that requires intrusive vs extrusive silicic rocks to have differing levels of radiogenic elements. Check into this again and update the reference if there is one.

Line 59. A gentle suggestion to use Compton Belkovich Volcanic Complex (CBVC) instead of C-B because the literature & lunar community seems to be coming to a consensus on using CBVC.

Line 59. If you aren't limited in number of references, it would be good to add Jolliff et al., 2011, and Clegg-Watkins et al., 2017 to the list of authors here as they both did detailed work on CBVC.

Lines 87-89. Clegg-Watkins et al., 2017 used photometry to determine that there may be andesite or dacite occurring at CBVC. This should probably be at least mentioned here.

Line 141. I suggest you change "higher than all but a few lunar samples" to specifically be about lunar granitic samples, as that's the key comparison you want to make. Maybe something like "Previous orbital measurements estimate C-B to have up to ~49 ppm Th, within the range of lunar granitic samples."

Line 164. I highly suggest using the instrumental neutron activation analysis (INAA) value (60 ppm Th) for this sample which is also given in Seddio et al., 2013. Given the nature of how the 132 ppm was calculated (modal recombination of EMPA data, for which the error bars would be significant for a trace element like Th) it is likely that the 132 ppm represents the bulk composition only of an anomalous region of the sample where a large fragment of a Th-bearing mineral (in this case, likely the unusually large zirconolite grain that occurs in the sample) happened to be and does not represent the real bulk composition of the sample. The 60 ppm value, however, was done by INAA and is thus representative of the whole sample. I understand that this has implications for your modeling, but 132 ppm Th is likely an unrealistic present-day Th value in lunar granite. If you disagree, I suggest that a stronger defense of using such a high value is needed in the manuscript.

Lines 164-166. Is there a terrestrial example of a 2-stage granitic magma chamber that has been modeled to be similar to this that you could compare to?

Line 185. I'd like to see a more in-depth discussion of why a 60-70 ppm Th model isn't consistent with the surface heat flux. The mismatch of what we're seeing in the samples and what you're seeing using heat flow is a really important point. How can we reconcile the two?

Lines 216-219. If you're up against length constraints, you can probably remove hypothesis 2. This has fallen out of favor and isn't really discussed much anymore.

Lines 218-219. Any discussion of granite formation mechanisms on the Moon should include a citation for Hagarty's (2006) basaltic underplating model. This was the basis for a lot of the discussion in the Gullickson 2016 paper cited here and is the currently favored model among petrologists.

Line 679-681. With a 60 ppm Th lower chamber, this would likely be a granitic lower chamber, so then what would the upper chamber be if it has evolved enough to have 2X the Th concentration? An already granitic magma does not have that much wiggle room in the interplay among major elements, temperature, and viscosity to continue to evolve before solidification. These factors should be considered when modeling silicic magma chambers.

Figures:
Figure 1 is missing at scalebar for Th?

*************************END*************************

Author Rebuttals to Initial Comments:

Dear referees,

Thank you for your extremely helpful and supportive commentary. We are excited to see this work move forward. We hope that we have made all the changes you had requested and please forgive the post-holiday delay in responses. Please find our responses below (in blue text) and follow "tracked changes" in the provided documents.

Thank you,

-Matt Siegler and team.

Referee #1:

Review of Remote Detection of a Lunar Granitic Batholith at Compton-Belkovich by Siegler et al.

I find this paper to be a compelling and exciting study where new results from the Chang'E-1 and Chang'E-2 Microwave RadioMeter (MRM) are presented for the Compton-Belkovich high thorium feature. In particular, evidence is presented to show there is likely a granite-like feature beneath the Compton-Belkovich feature. If true, this is a surprising result as granitic systems are rare beyond the Earth, and finding such a system on the Moon outside of the high-thorium Procellarum KREEP terrane would require some rethinking of how such processes could take place on the Moon.

While some of the details in this paper are beyond my area of expertise, in my reading of the paper I consider it to be generally sound, and should be published. I do have a number of comments/suggestions that should be considered by authors prior to any possible publication. Some comments are minor, and others are likely more substantive. My comments are given below in sequential order as they come up in the manuscript.

Thank you for the kind overview of our project. I am excited to see how things have come together and glad that our case has shown to be generally strong upon your expert review. We attempt to address your questions below and have updated the text accordingly. I hope the responses are acceptable.

1. Lines 56 – 58: Maybe reword to say: "… are generally coincident with high Th concentrations detected with orbital gamma-ray data [4,5]". The original wording is a bit odd.

Thank you, wording changed.

2. Line 59: The Lawrence et al. (2007) citation is not really the best one for discussing the original detection of the C-B anomaly. Rather, it is better to use Lawrence et al., GRL, 26 (#17), 2681, 1999 and Lawrence et al., JGR, 108 (#E9), 10.1029/2003JE002050, 2003. The Lawrence et al. (1999) paper provided the "discovery" result for the C-B anomaly, and Lawrence et al., (2003) provided the first estimate of the surface Th abundances of 40 – 55 ppm. This abundance estimate was later updated by Wilson et al., (2015) to be in the range of 14 – 26 ppm when using more realistic assumptions for a spatial reconstruction process. When quoting any orbit

estimated Th abundances, therefore the best value to use is this value from Wilson et al. (2015).

Thank you, we have added these references and updated all 49ppm references to 26 ppm.

3. Line 66: "… anomalously hot Apollo 15 site". Can a citation be provided for this number?

We have added the Langseth et al. reference appropriate here and removed the "anomalously hot" phrasing. Apollo 15 was just the highest measured heat flux value on the Moon as of current, but that is separate from any claim of it being anomalous.

4. Lines 67-68: " … much higher radiogenic element concentrations than previously observed from orbit." In reading later portions of the paper (Section 5 of the Methods section), it seems that this may be referring to the 60 – 132 ppm abundances mentioned there. It may be useful to be reminded that the orbit-measured data are limited by the fact that the spatial resolution is relatively broad, and there could be small regions on the Moon with higher abundances than actually measured. Such features though would need to be relatively small (such as what was assumed in Lawrence et al., (2003)).

Thank you. We fully understand that the derived surface concentration from the gamma ray data requires modeling of sub-resolution sources.
We have reworded this sentence as:
"Such high heat flux requires a large mid-crustal body with higher radiogenic element concentrations than previously interpreted from orbital observations"
But we can add further detail about the sub-pixel reconstruction of the gamma ray data as discussed in Lawrence et al., 2003 and Wilson et al. 2015 if you feel it is needed here.

5. In regards to Figure 1 (and other associated figures in the Methods section). This is a single, relatively small area on the Moon, and I am curious what similar data look like elsewhere. Is this location really as strange as suggested in the text? For example, earlier it is suggested that Apollo 15 has a measurable heat flow. Is this backed up with the data? Part of the reason that I ask is that in an analogous fashion, the C-B thorium enhancement would not be that remarkable if it were located in the PKT. However, the primary reason it really stands out is that it is a thorium enhancement in an otherwise very low-thorium portion of the moon. It is possible that this is a similar situation for the brightness temperature and thus heat flow? This is one area for which I am not an expert (I do not have a deep familiarity with the MRM data). So, if this is a naïve question, I defer to the experts. However, it does seem a question that a general scientist might ask when reading the paper.

This is exciting and indeed work that we are planning to present in a future paper. Yes, there are other regions which have elevated "data minus model" brightness temperatures. Compton Belkovich is the highest brightness temperature difference observed on the Moon, but more importantly for this interpretation, it is so elevated in brightness temperature that it can be observed as an anomaly at all frequencies (most others will only show elevated temperatures at 3 GHz) and it is in a region with very uniform loss properties. A very preliminary “Chang’E 2, 3GHz data minus model” plot is shown below. Here Compton Belkovich is bright, but lost a bit in topographic noise. In this paper, we processed all Compton-Belkovich data locally and included topographic models to clean the image up to that shown in Figure 1.

One can clearly see other areas (such as Aristarchus ~(30N,-50E)) are also very elevated in brightness temperature compared to expectations. In the case of Aristarchus, the roughly 5K TB increase at 3 GHz does not result in a large signal at 19 and 37 GHz, but furthermore, the antenna footprint of the Chang’E instruments includes both mare and “highlands” materials, which complicates the interpretation of the signal (as microwaves are emitted from far deeper in highlands material than mare, see Siegler et al., 2020). So, C-B was an excellent “first target” for this discovery paper on the topic as it was a large signal and the relatively uniform loss tangent over the region (Methods Fig 1) provided a very clean case for measurement interpretation.

At 3 GHz, only very high heat flux, low loss (highlands) areas show up as high brightness temperature. The Apollo 15 site, in a mare covered area with ~21mW/m^2 heat flux, does not have a large enough signal to observe. Truly mapping the heat flux of the Moon globally would require a lower frequency MRM type instrument (which we are currently developing at JPL) and helped support our proof-of-concept study with the Chang’E data that the Compton-Belkovich discovery is coming from. I personally think there is an exciting future of this instrument type and likely much more we can recover from the global data set… but nothing may be quite as exciting as the topics discussed in this paper. We have included a greatly cleaned-up Chang’E 1 and 2 global data set in the Supplemental Data section to allow the community further access to these data, but we have not yet personally processed the global dataset to the publication-ready state that the Compton-Belkovich region data has been processed. Stay tuned for this ongoing work to be reported at future conferences!

REDACTED

6. Lines 75, 78: The ‘MRM’ acronym is not defined in line 78, but could be conveniently defined in line 75.

Good idea- line 75 now reads:
“The Chang’E 1 and 2 orbiters carried four-channel (3-37 GHz) Microwave RadioMeter (MRM) instruments.”

7. Lines 96-102: When mentioning greater and shallower depths, it would be good to provide the notional depths that are represented here.
We have added
For MRM frequencies and lunar materials sounding depths are estimated at ~0.3 to 5.6m [16].
Referring to Siegler et al, 2020, summarized in Table 4 of that paper:

Table 4
Depth at Which the Weighting Function for Each MRM Channel Peaks and the Depth at Which It Receives 99% of its Energy for Highlands and 8% TiO_2 Mare

Channel	Highlands peak depth	8% TiO_2 mare peak depth	Highlands 99% depth	8% TiO_2 mare 99% depth
3.0 GHz	0.184 m	0.128 m	5.572 m	2.466 m
7.8 GHz	0.111 m	0.073 m	1.910 m	1.017 m
19 GHz	0.050 m	0.029 m	0.677 m	0.442 m
37 GHz	0.013 m	0.002 m	0.334 m	0.262 m

These depths are taken from the maximum sounding depth of 37GHz in mare materials and 3 GHz in highlands materials. The emitted radiation is a weighted sum of energy above those depths.

8. Line 110: Is the word ‘shallows’ correct? Should it be ‘shows’?

Thank you. Autocorrect + too many re-edits.

9. Lines 139-148:

As I mentioned earlier, I would not use the ~49 ppm Th concentration (and the reference of Lawrence et al. (2007) is not the correct reference for that value; rather it is Lawrence et al., (2003)), but rather the 14 – 26 ppm from Wilson et al., (2015). Of course, there could be localized (smaller than the GRS spatial footprint) values that are much larger, so there is some leeway here.

We had used the 49 ppm to show that even the highest plausible interpretation of materials suggested would not provide enough heat to reasonable fit the data. We agree that the 14-26 ppm revision is likely to be a better value to quote here. We just meant to show that we need a very high heat flux that is not accounted for from existing interpretation of surface data- so yes, there is likely to be some small amount of much higher Th material below the spatial footprint of LP-GRS.

As mentioned earlier, all 49ppm references have been updated using the 26ppm upper number.
This change also altered later line 143:

"180 mWm^{-2} would require a layer of roughly 45 km of such material, neglecting any lateral heat conduction, implying the material below the surface exceeds concentrations estimated at the surface."

Second, I'm not sure I fully follow the reasoning here about the heat flux. In this paragraph, it is mentioned there is a heat flux of 150 mWm-2, whereas in the 'Methods' section (line 665), the heat flux is given as ~180 mWm-2; I also note that in the abstract, there is a value of ~184 mWm-2 that is given. One further item that is not clear is how these values are arrived at. On line 81 and Figure 3a, there is a ΔT brightness temperature of ~9K; but when I then go to Figure 2c (the 'calibration' from brightness temperature to heat flux), I only get a heat flux of ~50 mWm-2. Maybe the larger number is arrived at when accounting for the broad spatial resolution of the microwave measurements? If this is the case, then it is not immediately clear to me how you got this value. So, if this explanation can be more clearly stated, that would be useful.

Thank you for this helpful comment:

We have simplified all mentions of heat flux to ~180 mW/m^2 to avoid confusion of our accuracy. The 150-200 mW/m^2 mention was put in as a "story telling" step as to how we came to the ~180 number, but ended up being confusing.

The second part of this comment appears to be a confusion between "Antenna temperature" and Brightness temperature". Brightness temperature is what we would see with an infinite resolution instrument- antenna temperature takes into account the antenna pattern. We discuss this in detail in Methods Section 2, but could expand this if it is unclear. The 9K is an increase in measured antenna temperature, TA. That will result in a much larger increase (~27K) in brightness temperature, Tb, for with the antenna model convolution. Instead of doing such a forward model of the data, we convolve our model antenna pattern with the heat flux. We use our simplified pluton model to create a roughly gaussian heat flux distribution on the surface. We could have simply fit a gaussian (as we did in trying to initially fit the shape. This required two gaussians (a tight one and a wide one), which lead to our initial examination of a 2-body pluton system.

We attempt to illustrate the somewhat subtle change between Tb and TA with an updated Figure 2, separating Figure 2d into two parts (now 2d and 2f) and adding Figure 2e showing the intermediate step of Tb (at the 4 Chang'E frequencies) that goes between converting a heat flux to the TA value for a specific antenna.

Also, please note, in addressing this comment we embarrassingly realized that we had used an outdated loss tangent model in Figure 2c which was not in fact the model we used in the paper (this was a co-author miscommunication).The old figure 2c was not consistent with the other plots or the 4 equations in Methods Section 2 and has now been updated.

Third, I do not follow the reasoning of how the measured thorium concentration is related to the thorium concentration in the putative column beneath the surface. It seems that the subsurface thorium is being scaled based only on the heat flux, and the surface expression of this thorium does not necessarily have to have the same value. Is there a good idea for why the surface concentration of the thorium would be much lower than the concentrations at depth? Maybe there is some dilution of the intrinsic subsurface thorium concentrations with lower values during the pyroclastic eruption that put the thorium on the surface? Is such an idea worth exploring here in the paper?

Certainly, the surface Th and the column Th abundance have some relation, we explore that in our recent paper for instance:

Siegler, M., Warren, P., Franco, K. L., Paige, D., Feng, J., & White, M. (2022). Lunar Heat Flow: Global Predictions and Reduced Heat Flux. *Journal of Geophysical Research: Planets*, *127*(9), e2022JE007182.

So, the crux of the argument for the Compton-Belkovich feature is "How are the Th and heat flux related and is C-B different than the general Moon?". We really are saying that the simplest assumption is that the column abundance of Th is represented by the surface value (explored globally in our 2022 paper). So the primary goal of that discussion in the C-B paper is designed to say "No, we need far more hidden Th to explain the heat flux values we derive." Also, please note that we derive a relative heat flux as compared to a location off of C-B (e.g. anywhere else on the Fig 1 map), so we are essentially able to say we need this much extra heat flux (scaled to a Th concentration) at CB relative to the average highlands neighboring C-B.

We are assuming that C-B's surface Th is a mix of erupted volcanic material (which can come from many depths within the volcanic system) and 3.5 Gyrs of regolith gardening. This first point is important as while some of the erupted materials could represent the silicic plutons we model, erupted material can also be dominated by the more mafic, and generally less viscous and lower Th, materials which were separated from the silicic material by the melting and fractionation that formed both reservoirs. So yes, the surface material of such a volcanic system is likely to underpredict materials below (granites are generally more radiogenic than volcanics), but we simply use it as a place to start.

This is addressed by the change in line 143:

"180 mWm^{-2} would require a layer of roughly 45 km of such material, neglecting any lateral heat conduction, implying the material below the surface exceeds concentrations estimated at the surface."

10. Lines 161-169: I want to poke a bit at the assumption that there should be two bodies beneath C-B. While it may make geologic sense to have two bodies (line 166), is there anything in the data that requires or explicitly points to such an idea? I ask because it naively seems simpler to just have one "average" body if that is all the data can constrain.

We could have added extended the discussion of the references (most explicitly : Chauhan, M., Bhattacharya, S., Saran, S., Chauhan, P., & Dagar, A. (2015). Compton–Belkovich volcanic complex (CBVC): An ash flow caldera on the Moon. *Icarus*, *253*, 115-129.), which have postulated the general existence of a 10-15km diameter body below the surface feature, based on surface fault structures, but felt that and other papers were referenced. Several papers referenced with that in our text (Jolliff, Hagerty, Clegg-Watkins…) support this conclusion. This is a very robust structural geology argument, detailed in summary papers like this:

Acocella, V. (2007). Understanding caldera structure and development: An overview of analogue models compared to natural calderas. *Earth-Science Reviews*, *85*(3-4), 125-160.

So, we are beginning with the well-justified assumption that there is a ~10-15km body (13km was taken as the favored diameter from Chauhan et al).

Next we fill it with as much Th as feasibly possible on the Moon. For this we chose 132ppm, based on the Seddio et al. work. Reviewer 2, and our own co-authors (and Steve Seddio, whom we contacted personally once we knew he had not been a reviewer for this paper), all agree that this is certainly an outlier for the Moon and may not be a feasible value to expect for a bulk granite composition (we will address this in detail in responses to reviewer 2 below). The use of the 132ppm was simply meant to show that even given this "crazy granite" (which was what Co-author Lehman-Franco and I have always called it), a 13km pluton could in no way create the heat flux signature we observe. This is illustrated in Fig 2D (upper) in which the needed heat flux profile for our fit is the yellow curve, while the 13km 132ppm Th pluton creates only the heat of the blue curve.

We then followed the argument that this extra heat needed to come from somewhere. Here the simplest and geologically justified shape of the second body was a ellipsoid. We chose the more representative ~69.7 ppm Th value (which is found in roughly 20 lunar granite samples) for this lower pluton. We then adjusted the size and depth of the lower pluton until we produced a forward model to match the observed heat flux values. We could have reduced the Th concentration of the relatively small upper pluton model at this point, but thought that would be confusing- so continued this as our "maximum case". What this is then revealing is that (under the assumption of ellipsoidal plutons), the minimum that a lower body could be is ~53km in diameter. If we lower the upper pluton Th, we need a larger lower pluton (~56-57km for an upper pluton with 69.7 ppm Th)….We can show a lot of model space, but chose to keep the story as simple as possible by providing an end member case here.

We believe that this modified outline of the concept and logic addresses this point and may also resolve the concern at the heart of Reviewer 2's substantive "Line 164" comments, which we wholly agree with.

Essentially, the two pluton model is not meant to be "the 100% correct model", but a working model that shows that even given the most extreme possible assumptions of the generally accepted upper pluton composition, the large underlying batholith. This most extreme case puts a large amount of Th/U/K in the upper pluton and therefore minimizes the size of the lower pluton.

We have substantially reworded the paragraphs covering lines 140-191 to address this and comments from Reviewer 2. We have also added Methods material and a new Figure 9 in the Methods to address this. We believe that these now help resolve your concerns.

11. Figure 4 seems to be missing a ‘c)’ label (at least in my copy).
Thank you. There was a last-minute figure change that resulted in an error.

12. Methods section, line 468: “ … appears justified …”. If I understand the text correctly, this is claiming that in Extended Data Figure 1, the red data points are well fit by the blue line, which is the model. However, two of the four data points do not fit the model within the error bars, and the other two points fit the model, but through large error bars. In fact, one might argue that a horizontal line would provide a better fit to the red data points (by eye, I can see that a horizontal line can go through the error bars of all four data points). Thus, is the claim “appears justified” really justified? Or am I misunderstanding what is being said?

The justification is meant to reflect the global spread of loss tangent values as discussed in:
Siegler, M. A., Feng, J., Lucey, P. G., Ghent, R. R., Hayne, P. O., & White, M. N. (2020). Lunar titanium and frequency-dependent microwave loss tangent as constrained by the Chang'E-2 MRM and LRO diviner lunar radiometers. *Journal of Geophysical Research: Planets*, *125*(9), e2020JE006405.

We have changed the text to clearly cite this:
“Overall, the average highlands model (fit to global data) appears justified based on the Siegler et al. [16] for use over this entire region.”
While a straight line fit would potentially also work for this small data set, we might as well use the knowledge that the previous work on the global data set has provided. A straight line fit would have also in this case been higher loss at 3 and 7.8 GHz, meaning we would need an even higher heat flux to fit our data.

13. Methods section, line 520: What is the nature of the latitude correction? This correction is mentioned a few times in the paper, and it is not clear what is being done for the correction.

This is a roll over from something we did in the forementioned Siegler et al., 2020 paper. MRM data is foremost related to physical temperature, which leaves a latitudinal trend. This is seen in Extended Data Figure 3b for instance. We then remove this trend, normalizing to the temperatures at the center latitude of out map (vertically), which is 61N in this case. So all “delta T values are in reference to that average.

The first mention is in line 80 so we have added
“**Figure 1** shows 3 GHz midnight antenna temperature in the C-B region (after latitudinal trends have been removed and normalized to 61ºN), revealing an enhancement of ~9K, coincident with the Th-anomaly.

We have also added:
“Note the trend in temperatures with latitude”
into the Extended data 3b figure caption to highlight this.

We believe that his helps resolve this question.

14. Methods section, line 556: "… at all local time …": 'time' needs an 's' to make it 'times'.
Thank you.

15. Methods section, Extended Data Figure 3: In my copy, this figure is missing the a), b), and c) labels.
Thank you.

16. Extended data figure 4: This figure seems to be missing? (maybe it is the figure around line 630?)
This was an error. We did not realize the 10 figure limit for the Methods section until shortly before submission and lost this in the shuffle of figures. This should not be fixed and allowed us to add a new Extended Data Figure 9 and still remain below the limit.

17. Extended data figure 7: This figure appears to missing the labels?
Added "(units K)" to caption if this was the issue, which we believe was the comment's intention.

18. Methods section, line 663: "… about 7 ppm from the pixon reconstruction …". You might want to note that the actual inferred thorium abundances at the surface (according to Wilson et al., (2015)) is 14 – 26 ppm.

We rewrote this text as:
"However, the directly derived Th concentration (about 7 ppm from the pixon reconstruction, **Extended Data Figure 7e**) and even the remodeled surface Th concentration (according to Wilson et al., [6] is 14 – 26 ppm) is far too low of a radiogenic concentration to explain the ~180 mWm^{-2} heat flux required by our data."

19. Methods section, lines 694 – 697: "The presence of a more evolved upper body suggests …". As I suggested in a comment earlier (and will ask again here), is the existence of two bodies really proven or shown by the data, or is this conclusion speculative? Or to take in another way – would the data fit a single body or is there a data driven way to distinguish between the two scenarios?

Basically, a single body could explain the data, but it would need to be large (~55-60km with ~69.7 ppm Th), deeper than would impact the observed surface geology, and it would need to have a more complicated shape than our ellipsoid model. Since the structure of the caldera provides a strong constraint on the existence of a ~13 km upper body and we cannot create enough heat from that body (even when we put a ridiculous amount of Th, ~132ppm, in it)- we call on the existence of the lower, larger body to explain this. This is not unique- we could have 10 lower bodies for all we know, but it is a model that is both consistent with the surface geology and the microwave data. The big discovery here is that the upper pluton (that has been theorized before by Chauhan, Jolliff, Wilson and Head, and others) is not nearly big enough to create the surface heat flux we observe. We have rewritten lines 140-191 substantially and added Extended

Data Figure 9 and related discussion to address this.

-David Lawrence

Thank you for the extremely useful comments, David, and thank you for providing your identity for placing the comments into context. The review has been very useful in understanding where we needed to explain things more clearly and I hope that you find that it is improved.

Thank you again Dr. Lawrence for the very useful commentary.

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Referee #2:

Dear Authors,

Congratulations on your completion of a very interesting and engaging manuscript. Given the recent interest in silicic volcanism on the Moon within the lunar community this research will add a significant and valued piece of the puzzle that is silicic volcanism. Little is known about the underlying magma chambers that feed silicic domes and your work will help to fill that knowledge gap. I do, however, strongly urge you to consider my comments about the chosen Th values for your model, discussed in detail below. I look forward to seeing the finished manuscript and discussing your work with you at future conferences.

Regards,
-Dr. Sarah Valencia

Thank you Dr. Valencia,
I am also excited to discuss this work with you in the near future too and thank you for the extremely useful comments. As my student/co-author (Katelyn Lehman-Franco) works forward on the interpretation of this for her recently funded FINESST proposal, it would be great to have you as a useful guide for her work. We agree with you about the interpretation, especially of the 132 ppm Th number, which Katelyn and I refer to as "crazy granite", so I believe the general comments you bring up there are much more a matter of clarity in our presentation. While "A-type" granites on Earth achieve these values (we did not want to open up the "A-type" classification in this paper as it is itself apparently controversial) on Earth, we agree that most granites are likely to fall below this concentration. We therefore have restructured that discussion to illustrate that taking the 132 ppm Th concertation as an end-member model lets us set a potential minimum size and concertation for the lower pluton… essentially proving that it needs to be quite large to fit our observations.

As discussed in comments to David Lawrence, above, we simply meant to state that: under the assumption that an upper pluton exists (as has been hypothesized by other authors), ever filling it with the most radiogenic material on the Moon (~132ppm Th) would not provide enough heat production to explain our signal. Therefore, we need a second, much larger body.

You rightly point out that once we add that second body, the upper body need not be 132 ppm. We left it as that in our nominal model to serve as an end member. Essentially, by packing as much heat as possible into the upper pluton, it minimizes the volume of the lower pluton.

We have rewritten lines 140-191 substantially and added Extended Data Figure 9 and related discussion to address this. Extended Data 9 shows example fits for an upper body with "G_B" (69.7 ppm Th) and zero heat production to illustrate that the exact value of "Gc" (132 ppm Th) is not something that we fit or truly need. We do see in this comparison that the Gc upper pluton model is a somewhat better fit than the G_B upper pluton model, which is generally consistent with the real concentration being somewhere in between. We could have run an additional model space parameter with varying upper pluton Th values, but we would still be relying on assumptions of the shape of the plutons and the lower pluton/batholith radiogenic concentration which limit the uniqueness of this fit.

We have switched all mention of our "preferred" model to be of our "end-member" model, referring to the 132 ppm model as clearly being an end member case of "you couldn't possibly imagine more Th than this in the upper body" which will result in a minimum volume for the lower body. Rewriting it this way is more satisfying in that we then get ~53 km as a minimum lower pluton size (assuming it is 69.7 ppm Th) and ~60 km as a maximum size (assuming zero heat production from the upper body- Extended data Figure 9b). That provides a very nice testable constraint if we could land and measure the size of this body with geophysical techniques as mentioned in the conclusions.

Lines 53-54. Seddio et al. 2015 looked at the petrogenesis of intrusive vs extrusive granitic rocks by looking at the textures and Raman spectra of silica phases, not at the concentrations of radiogenic elements. I don't personally know of a mechanism that requires intrusive vs extrusive silicic rocks to have differing levels of radiogenic elements. Check into this again and update the reference if there is one.

Rereading this sentence, it seems the mention of extrusive volcanism is confusing here, so we have simply removed: "suggested to represent extrusive volcanism".

Line 59. A gentle suggestion to use Compton Belkovich Volcanic Complex (CBVC) instead of C-B because the literature & lunar community seems to be coming to a consensus on using CBVC.

This was a bit of a hold-over from our early drafts of this paper before I personally had read much of the literature on the topic. I interpreted CBVC as refering to a smaller area of the feature. Also, in attempting to save word count for the Nature format, "C-B" was shorter than "the CBVC"… but I agree that it is a reasonable change and has been fixed throughout the text.

Line 59. If you aren't limited in number of references, it would be good to add Jolliff et al., 2011, and Clegg-Watkins et al., 2017 to the list of authors here as they both did detailed work on CBVC.

Lines 87-89. Clegg-Watkins et al., 2017 used photometry to determine that there may be andesite or dacite occurring at CBVC. This should probably be at least mentioned here.

Thank you, that is a good addition and we apologize for missing that reference in the literature.

Line 141. I suggest you change "higher than all but a few lunar samples" to specifically be about lunar granitic samples, as that's the key comparison you want to make. Maybe something like "Previous orbital measurements estimate C-B to have up to ~49 ppm Th, within the range of lunar granitic samples."

Thank you, we have adopted your new wording, but reviewer 1 has suggested that ~26ppm Th is a better interpretation of the orbital data. This is not as valid of a statement for making the CBVC clearly granitic (26 is actually lower than most of the "granites"), but it is well understood that the ~60 km footprint of the LP-GRS measurement is likely to underpredict erupted material which could be confined to specific outcrops that might be higher Th concentrations.

Line 164. I highly suggest using the instrumental neutron activation analysis (INAA) value (60 ppm Th) for this sample which is also given in Seddio et al., 2013. Given the nature of how the 132 ppm was calculated (modal recombination of EMPA data, for which the error bars would be significant for a trace element like Th) it is likely that the 132 ppm represents the bulk composition only of an anomalous region of the sample where a large fragment of a Th-bearing mineral (in this case, likely the unusually large zirconolite grain that occurs in the sample) happened to be and does not represent the real bulk composition of the sample. The 60 ppm value, however, was done by INAA and is thus representative of the whole sample. I understand that this has implications for your modeling, but 132 ppm Th is likely an unrealistic present-day Th value in lunar granite. If you disagree, I suggest that a stronger defense of using such a high value is needed in the manuscript.

As noted in the first section of our response, this is a very important point and we agree fully. We had meant this to be a proof that even with the upper pluton (which people have hypothesized for ~15+ years and we would argue is generally accepted to exist) composed of the most Th-rich material ever found on the Moon, we could not come close to the amount of heat production needed to explain our signal. This lead us to our key scientific finding that CBVC is underlain by a larger magmatic system, which we have simplified as a single lower, ellipsoidal pluton.

In agreement with your important point, we have rewritten lines 140-191 substantially and added Extended Data Figure 9 and related discussion to clarify and address this. We agree that the upper body does not need to be 132 ppm Th, we just set this as an upper limit case. We do find that this fits the data better than the upper pluton being ~70ppm or lower, but we have not done an exhaustive fit as this fit is dependent on many model assumptions (namely of the shape and radiogenic concentration of the lower body). We thank you for helping us to clarify this point.

Lines 164-166. Is there a terrestrial example of a 2-stage granitic magma chamber that has been modeled to be similar to this that you could compare to?

In line 63, we compare this in size to the Andean Altiplano-Puna Magma Body, but do not make any attempt to further compare this with an Earth system. In the sense of mapped heat flux, this has not been done for many volcanic regions as it would be very costly to drill a suitable heat flux grid. I have seen heat flux maps of Yellowstone that provide some sense of structure of hot materials below the surface, but this is a rather different scenario. This is part of the importance of ability to use microwave radiometry to map geothermal heat flux. This is a novel ability we have never had on Earth due to the prevalence of liquid water, which has an incredibly high loss and prevents microwave emission from depth. An orbital or rover-based microwave radiometry system on any body without liquid water (such as the Moon, Mars or icy satellites) would provide a new look into potential magmatic or cryo-magmatic systems throughout the solar system. We hope that this important point is now made in the revised manuscript.

Line 185. I'd like to see a more in-depth discussion of why a 60-70 ppm Th model isn't consistent with the surface heat flux. The mismatch of what we're seeing in the samples and what you're seeing using heat flow is a really important point. How can we reconcile the two?

In order to address this important point, we have added material, especially in the the methods section, to discus this. We can trade off a lot of attributes in any model. Here, less radiogenic material in the upper pluton would lead to a larger lower pluton- to an extent. We found a better fit with a higher radiogenic content in the upper pluton, which could point towards CBVC being more radiogenic than samples that have been returned, which would not be surprising as this system appears to be quite unique with evidence for multiple distillation chambers over a much thicker crust (50km vs ~25) than that found in nearside systems- however, we agree that this is speculative. We have added a sentence that makes this point, and indicates that Compton-Belkovich is among the top candidates for future surface exploration (Jawin, E. R., Valencia, S. N., Watkins, R. N., Crowell, J. M., Neal, C. R., & Schmidt, G. (2019). Lunar science for landed missions workshop findings report. Earth and Space Science, 6, 2–40. https://doi.org/10.1029/2018EA000490); hopefully we can collect or measure samples at the CBVC in the nea future.

Lines 216-219. If you're up against length constraints, you can probably remove hypothesis 2. This has fallen out of favor and isn't really discussed much anymore.

Thank you, for sake of completeness, we have left this in. Looking at the gravity data (e.g. figure 4 e,f), it is curious that CBVC is on the edge of a "roundish" area of thinned crust, this could be evidence of some form of impact at an early stage of crustal formation (or thinning of the crust by a plume), which could be construed as relating to hypothesis 2. This could be a plausible defense for leaving it in there. We do note in what is now line 234/5:
"The system's geometry required to accommodate the observed heat flow anomalies precludes Hypothesis 2."

Lines 218-219. Any discussion of granite formation mechanisms on the Moon should include a citation for Hagarty's (2006) basaltic underplating model. This was the basis for a lot of the discussion in the Gullickson 2016 paper cited here and is the currently favored model among petrologists.
Thank you, we have added this reference.

Line 679-681. With a 60 ppm Th lower chamber, this would likely be a granitic lower chamber, so then what would the upper chamber be if it has evolved enough to have 2X the Th concentration? An already granitic magma does not have that much wiggle room in the interplay among major elements, temperature, and viscosity to continue to evolve before solidification. These factors should be considered when modeling silicic magma chambers.

Thank you, this is an important thing to include in future modeling work (which is essentially Co-author/grad student Lehman-Franco's thesis proposal building off this "discovery" paper. We have not attempted here to develop a fully functional petrologic model. She has been working with "Rhyolite MELTS" https://melts.ofm-research.org as a starting point for this work, which will hopefully be productive. A large question is the potential water available within the system, which will greatly impact the melting temperatures and compositions. It is a reasonable question as to whether a factor of 2 increase in concentration is reasonable- it essentially requires a small degree of partial melting, which would require a slow and steady heat source over a long time (such as a mantle plume or self-heating from radiogenic materials). We have only cracked the surface of what can be determined about the CBVC system, and it will be exciting to see what lies ahead.

Figures:
Figure 1 is missing at scalebar for Th?

Thank you, this was meant as a visual reference to orient the reader to the CBVC's farside location, we have "added *scaled 0-35 ppm Th" in the figure caption to address this.*

Thank you for these incredibly useful comments. We agree that the paper is better served without saying 132 ppm granite, which we also discussed with Steve Seddio. We hope the wording as this as an end-member and therefore a lower limit for the heat production (volume and radiogenic concentration needed) from the lower pluton. Looking at it this way offers a more robust suggestion of a ~53-60km body (under the 69.7ppm Th lower body value), which we feel is a nice way to summarize our result.

We hope that everything else has been addressed and will certainly look forward to discussing ramifications of this work with you in the future.

-Matt Siegler and team.

Reviewer Reports on the First Revision:

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Referee #1:

I have reviewed the responses from the authors in regard to both reviews, and I am satisfied they have addressed all my comments; I see no barriers to this paper being published.

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Referee #2:

Dear Matt and Team,

Thank you for your responsiveness and in-depth replies to my comments on the previous draft of the manuscript. I have just a couple of small comments listed below. Your reframing of the Th concentrations used in the models in this version of the manuscript was highly successful. That portion of the manuscript flows much smoother now. I'm excited to see this work move forward as it adds a valuable contribution to the confusing world of lunar silicic volcanism. I am also very happy to hear that Katelyn has funding to continue working on the petrological implications of the model. I look forward to following the work. Congratulations on getting this wrapped up!

-Sarah Valencia

Line 151-152. This isn't quite true. High-Th impact melt breccias have Th up to ~33 ppm (see Korotev et al. 2011, Apollo 12 revisited). You could add a qualifier somewhere in this sentence that specifically any igneous rocks >26 ppm Th are quartz monzodiorite or granite.

Line 232-234. Also of note is that silicate liquid immiscibility is precluded at CBVC because sample studies and experiments demonstrate that SLI does not produce a large enough volume of the silicic phase to be able to produce large constructs. This isn't critical to include, just more of an fyi in case it comes up.

********************************END********************************

Author Rebuttals to First Revision:

In response to reviewer #2 (Sarah Valencia)

Line 151-152. This isn't quite true. High-Th impact melt breccias have Th up to ~33 ppm (see Korotev et al. 2011, Apollo 12 revisited). You could add a qualifier somewhere in this sentence that specifically any igneous rocks >26 ppm Th are quartz monzodiorite or granite.

We have updated the text to read the following:

Based on returned igneous lunar samples, only quartz monzodiorite and felsite/granite samples have higher than 26 ppm Th.

Line 232-234. Also of note is that silicate liquid immiscibility is precluded at CBVC because sample studies and experiments demonstrate that SLI does not produce a large enough volume of the silicic phase to be able to produce large constructs. This isn't critical to include, just more of an fyi in case it comes up.

We have updated the text to read the following:

Hypothesis 1 is precluded at the CBVC due to the large volume needed as well as lack of correlation between high Th contents and a large positive Bouguer gravity anomaly, as Th would fractionate into the denser, iron-rich component [36].

Thank you and the reviewers for the very thoughtful and helpful process in the production of this manuscript. Please let us know if any of the files need updating or further edits or needed.

All the best,
-Matt Siegler.