Peer Review File

Manuscript Title: A dense ring of the trans-Neptunian object Quaoar outside its Roche Limit

Reviewer Comments & Author Rebuttals

Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

A: Summary of the key results

The manuscript "A dense ring around the trans-Neptunian object (50000) Quaoar well outside its Roche Limit by B.E. Morgado et al. describes evidence from a series of stellar occultations that there is a ring of material around Quaoar. This ring is particular interesting because it is located so far from Quaoar that it should be outside the classical Roche limit, implying that some dynamical process is preventing this material from aggregating into a moon.

B: Originality and significance: if not novel, please include reference

These findings are novel and should be interest to a broad interest to the planetary science community. While planetary rings have been previously found around small bodies, this very distant ring of material is unexpected and suggests that we need to re-evaluate how quickly orbiting material will coagulate into larger bodies.

C: Data & methodology: validity of approach, quality of data, quality of presentation

The evidence for this new ring appears to be solid. Stellar occultations are extremely sensitive to these sorts of narrow rings, and this team has extensive experience in observing these events and processing the resulting data. The relevant data are clearly illustrated and described.

D: Appropriate use of statistics and treatment of uncertainties

Overall, the treatment of uncertainties is reasonable. There are a few things that the authors should clarify about these uncertainties that I provide under point F, but these should be relatively easy to addres. The one general point I would make is that it is not clear whether the errors provided in Table 1 and Supplementary Table 5 correspond to one sigma or three sigma uncertainties. This should be explicitly stated somewhere in the paper.

E: Conclusions: robustness, validity, reliability

The conclusion that Quaoar has a ring of orbital debris appears to be very robust, given the presented evidence. This ring is also reliably well outside the Roche limit. The idea that this is because the velocity dispersion of the ring particles is high enough to inhibit ring material from accreting into moons is also a reasonable explanation for this finding. I do have some comments about some of the author's specific arguments about the ring particle size distribution, the mechanisms for maintaining the high velocity dispersion, and the potential connection with a spin orbit resonance, which are provided under point F. These should therefore be reasonably straightforward to address.

F : Suggested improvements: experiments, data for possible revision

I do have some suggestions regarding the data analysis and interpretation of the data. None of these should require the authors to do any additional analysis. They are mostly requests to either provide some additional information about their calculations or to revise/clarify some of their arguments.

Specifically with regards to the data analysis, I have three suggestions.

First, as mentioned under point D above, the authors should clarify whether the error bars provided in Table 1 and Supplemental Table 5 correspond to one sigma or three sigma uncertainties.

Second, while the authors provide estimates of the uncertainties on the equivalent width and depth for the positive ring detections, the authors do not provide explicit upper limits on these quantities for most of the occultations with no clear detections (the exception being one case discussed in line 469-475). Having a table of upper limits from the non-detections would be useful for assessing the true range of variations in this ring and comparing it to other rings. For example, the equivalent depth of the F ring ranges between 7 km and 19 km in stellar occultations (see French et al. 2012 Icarus), which is a comparable fractional range to the detections reported in this paper. This would support the comparisons between the two rings made by the authors in line 60 ff. However, if the times the ring was not detected required the equivalent depth of the ring to be much smaller than this range, then it might be worth comparing this ring to Neptune's Adams ring as well. In addition, there appears to be a dip at 80 second in Figure 1f that doesn't look dramatically weaker than the dip seen in Figure 1e, but the caption says that "no definite detection is made.... only an upper limit is derived". Knowing what the upper limit actually was would help the reader to understand why the latter is not a definite detection. I would therefore ask the authors to provide explicit upper limits on the ring equivalent width or depth for the profiles where the ring is not clearly detected. This could be included in one of the supplementary tables.

Third, while I agree the assumption of uniform opacity rings is sensible for most of the profiles discussed in the paper, the ingress profile from the HiPERCAM occultation appears to show some substructure. In particular, there appears to be a denser core around -148 seconds and maybe a weak gap around -145 seconds. I do not think the authors need to do a detailed analysis of these potential structures in this paper, but the authors should clarify in either Section 3 or 4 of the Methods whether this could affect their model fit parameters, particularly the equivalent width and depth. This would allow the reader to have a more secure understanding of what the parameters given in the tables represent for that particular cut through the ring.

I also have some concerns about some aspects of the authors' interpretation of these observations. Specifically, I have issues with the author's statements about the particle sizes in the ring, their discussion of the mechanisms whereby the rings might achieve a high velocity dispersion, and the presented evidence that the ring might be in a spin-orbit resonance for Quaoar. Note that none of these issues reflects a fundamental problem for the paper, instead they are all matters that can be addressed by adjusting some of the text or by providing some more details about their analysis.

First, the authors make some statements about the likely sizes of the particles in the ring that are not appropriate. In particular, the authors say in lines 57-58 that "our numerical integrations show that particles smaller than a few centimeters should escape the ring over a few years timescale due to radiation pressure" However, Methods Section 9 never mentions particles in the few centimeter size range. Instead, this section says that according to Formula 16 "particles smaller than 0.2 cm suffer radial excursions that surpass the ring width" and that only particles smaller than 45 microns collide with Quaoar. They also say that in their simulations particles less that 45 microns "do not survive over a few decades. These statements therefore say that the particles lost to solar radiation pressure are smaller than 100 microns, not a few centimeters. Furthermore, Equation 16 also almost certainly overestimates the eccentricities induced by solar radiation pressure because that particular expression neglects any apsidal precession induced by either

Quaoar's J2 or Weywot, which will tend to limit these eccentricities. Also, even if the lifetime of individual sub-centimeter grains is few years, there can still be a steady-state population of such grains released by larger objects (as the authors themselves say on line 65). Thus the authors have provided no compelling evidence that the ring is "mainly composed of macroscopic (>1 cm particles)" (lines 67-68).

While the above contradiction needs to be rectified, I agree that the ring contains a substantial population of particles larger than a centimeter in radius. I think the issue is that the different statements are creating confusion because they do not clearly distinguish between the particles that produce the signals and the particles that dominate the ring's overall mass. In this context, it is worth considering the F ring. That ring does contain a population of macroscopic particles, but most of the signal in stellar occultations of this ring actually comes from particles in the 1-100 micron size range. As the authors state in lines 63-65, the individual particles in this size range are expected to be relatively short-lived, but there is still a large number of these particles because collisions into and among the larger objects in this ring are constantly releasing new particles into the ring.

I therefore think the authors could address this issue by removing the references to cm-scale particles in the main text, as well as the text saying the ring is "mainly compared of macroscopic particles". Instead they could just say that isolated particles <100 microns will be lost on time scales of years, so there is likely a population of macroscopic particles within this ring.

Second, regarding the interpretation of these data and how a dense ring could exist this far from Quaoar, I agree with the authors that this is most likely because the velocity dispersion of the ring particles is high enough to prevent particles from accreting efficiently. However, the paper does not clearly articulate that there are multiple ways this could happen. After pointing out the need for a large velocity dispersion in line 95ff, the authors make a connection between this dispersion and the particles' coefficient of restitution and focus exclusively on the idea that the collisions in this ring might be sufficiently elastic to avoid aggregation for the next 4 paragraphs. After considering this option, they go on to discuss how external perturbations could also affect this ring, but at this point they also start talking about how the ring is confined. Thus while they mention that resonant perturbations could also potentially increase the velocity dispersion in the ring, the discussion does not make it clear that external perturbations may be a viable alternative to achieving the high velocity dispersions needed to keep the ring material from aggregating.

I therefore suggest that the authors clearly state around line 98 that there are two ways the material in a ring can achieve the high velocity dispersion required to maintain the ring well outside the Roche limit. One being the collisional coefficient of restitution, and the other involving the ring material being perturbed by outside forces. Having both possibilities presented up front would make the following discussions easier for the reader to follow.

In addition, I would urge the authors to also consider the possibility that a nearby small moon could also produce perturbations that would allow the ring to persist outside the Roche limit. Numerical simulations of Saturn's F ring already show that gravitational perturbations from the nearby moon Prometheus can generate wide ranges of velocity dispersion at different locations around the ring (see Beurle et al. 2010 ApJL and Sutton and Kusmartsev 2016 EM&P). Much of this work focused on regions of anomalously low velocity dispersion that could favor the formation of small moonlets, but the same simulations also show regions where the velocity distribution gets well above 50 cm/s, which may be enough to disrupt loose aggregates. Thus this also seems like a reasonable explanation for how the ring exists this far from Quaoar.

To be clear, I am not saying the authors need to do additional simulations of perturbed rings. I think the simulations of unperturbed ring regions are reasonable for this paper, as they provide good quantitative estimates of what sorts of velocity dispersions are needed to avoid runaway accretion. Simulations of perturbed rings would involve a much broader parameter space that is beyond the scope of this work. I therefore agree with the author's statement in lines 160-170 that this should be the subject of future work.

Finally, the authors present evidence that the ring material may be close to a 1/3 spin-orbit resonance. While this is certainly an intriguing possibility, the authors need to clarify some aspects of their evidence that the resonance is likely to fall close to the ring.

Specifically, in Table 1 the authors state the mass of Quaoar is 1.20+/-0.05 x 10^21 kg. This is actually a new constraint on Quaoar's mass, and is slightly lower and tighter than the previously published number of 1.3-1.5 +/-0.1 x 10^21 kg provided by Fraser et al. 2013. Note that if one uses the central value from this earlier work, the resonance location moves out to 4420 km, thus the case for the resonance's proximity to the ring is considerably stronger with this new estimate of Quaoar's mass. In Methods Section 1 the authors state that this mass comes from a fit done to the same Keck observations as those used by Fraser et al. 2013 along with one occultation from 2019, but they do not explicitly state what caused the central value to shift or the error bars to become tighter. The authors therefore should clarify whether their lower value for Quaoar's mass is due to including the occultation data, or instead has something to do with using the GENOID technique on the Keck observations. Also, the authors should clarify why they included that one particular occultation, and not other occultations like those described in Braga-Ribas et al. 2013. This information is needed for any future scholar who wants to investigate the role of this resonance in more detail.

Also, in Table 1 the authors give Quaoar's rotation period as 8.8494 hours, but also say the preferred rotation is 17.6788 hours. However, they don't explain why this is the preferred solution in the main body of the paper. In line 555 of the methods they clarify that this solution corresponds to a double-peaked rotation curve. This is a choice, but as far as I am aware there is not yet any independent evidence that favors the 17.6788 hour solution over the 8.8494 hour solution. Thus stating the 17.6788 hour is "preferred" seems to just mean that the author's prefer it because it places the stronger 1/3 spin-orbit resonance near the ring, whereas if the 8.8494 hour solution was correct, the ring would be close to a weaker 1/6 spin orbit resonance. If there is independent evidence that the spin period is the longer one, the authors should point that out explicitly, and if not, the authors should clarify that point. This could be done in a footnote to Table 1, as well as in Methods Section 6.

My remaining comments are mostly relatively small matters of wording and clarification, which are given below based on line numbers.

Line 18: "All the dense collisional rings observed so far reside inside the Roche limit of their central body...." This sentence may require some adjustment, since there are dense rings (most notably Saturn's outer A ring and Uranus' epsilon ring) that reside close to the Roche Limit itself. I would suggest instead something like "All dense rings are located close enough to their central bodies to be inside the Roche limit where tidal forces would prevent material with reasonable densities from aggregating into a moon."

Line 23: "thus questioning the validity of this limit" I feel this particular wording is too vague, since it could appear to suggest that the limit has been computed incorrectly, whereas in fact what they argue is that the limit may not always be valid, depending on the physical properties and dynamical environment of the relevant material. I would suggest instead phrasing this to be something like "thus implying that this limit does not always determine where ring material can survive."

Line 30: "(about half the size of Pluto)" could be "(about half the radius of Pluto)" to be more specific.

Line 33: "stellar occultations by this body since 2011." Is the "since 2011" needed? The occultations discussed here all come from after 2018.

Line 56: While Bosh et al. 2002 is the best reference for the lack of spectral variations in visible occultations, it may be worth noting that lack of spectral variations in optical depth in Saturn's rings was also reported in the near-infrared by Harbison et al. (2013 Icarus) and in the Ultraviolet by Becker et al. (2018 Icarus).

Line 58-59: This statement about the composition of the ring being potentially water ice seems out of place here, since the rest of the paragraph is about the particle sizes, not composition. This probably should be deleted since the composition of the rings is not really discussed elsewhere in the paper.

Line 147: The authors should probably clarify (either here on in Methods Section 6) whether a hemispheric mountain model is compatible with the assumption of the double-peaked rotation curve (which would imply a primarily m=2 anomaly in the gravitational field).

Line 187: "thoroughly" maybe should be deleted, since it still works for most ringed objects.

Line 210: Reference 10 should probably be to the Icarus article from 2013, not the AAS abstract from 2012.

Table 1: The "Date and Station" column for the occultation is confusing and not completely consistent with the rest of the paper.

First of all, there are parenthetical notes "(bef.)" and "(aft.)", but these are not defined anywhere. i think they mean "before" and "after" the body occultation. These terms should be explicitly defined in a footnote.

More importantly, the names for the "Station" do not match the names used in the figures. For example, the Table uses "GTC" for what is called "HiPERCAM" in Figure 1, and "Langersek" for what is called "Algester" in that same figure. I could figure out which was which using Supplementary Table 1, but this did complicate efforts to cross-correlate the numbers in the table with the visual appearance of the occultation dips. The authors therefore need to ensure that the "Stations" given in Table 1 and Supplementary Table 4 match the names provided in the various figures.

Table 1 also includes the normal optical depth and equivalent width of the ring, but omits opacity and equivalent depth This is not a big deal since all the parameters are given in Supplementary Table 4, but since all four parameters are discussed in the text, they probably should all be included in this table, if possible.

Figure 2: I was initially confused by the 11 June 2020 image because the two cuts were not parallel to each other, which is typically the case for different observations of the same occultation (see for example the 27 August 2021 event). I understand this happened because one of these observations was by the CHEOPS space telescope. However, to clarify the situation for the general reader, I do think it would be worthing briefly mentioned this explicitly in the figure caption.

Figure 3: I found the designations "Model 1-4" a bit opaque on first reading. Since the main difference between these models is how the coefficient of restitution varies with impact speed, I would suggest adding the explicit formula for these models to Figure 3 so that the reader does not have to go to Supplemental Figure 8 to track those down.

Line 469: "(27 October 2021)" should be "(27 August 2021)"

Line 477: "None of [the prior occultations] provided data with sufficient quality to detect the ring"

This statement is a bit surprising, since the S.P, Atacama light curve from Braga-Ribas et al. 2013 looks like it might have been high enough quality to detect the ring, did this occultation simply not cover a wide enough timespan around Quaoar?

Equation (11) and below. Given epsilon is elsewhere used as the coefficient of restitution, It might be good if the authors would use another symbol besides epsilon_k for the coefficients on the terms in the Hamiltonian. If this is not possible, please include an explicit statement that these coefficients are completely unrelated to the restitution coefficients.

Line 593-594. In the equation for the Jacobi constant, it might be useful to have a second equality where e^2 is re-written as X^2+Y^2.

Line 598: "It is proportional to mu_Q or mu_W". Since mu_W has not been defined yet, and mu_Q is defined in the main text, I would suggest saying instead something like "It is proportional to the normalized mass of the mountain (mu_Q) or Weywot (mu_W)."

Supplementary Table 5: If possible, I might suggest adding explicit references to References 44 and 45 for the coefficients.

G. References: appropriate credit to previous work?

References are generally appropriate. I make a few suggestions for potential additional references under point F above.

H. Clarity and context: lucidity of abstract/summary, appropriateness of abstract, introduction and conclusions

The paper puts these results in context very well, the abstract and introduction are both clear and lucid. The conclusions are reasonably clear, up to the various suggestions made under point F.

Referee #2 (Remarks to the Author):

Review of Morgado et al., "A dense ring around the trans-Neptunian object (50000) Quaoar well outside its Roche Limit."

A Summary of the key results
This brings the number small bodies with rings from two to three.
Each are remarkable. This one is remarkable largely for its location.

B Originality and significance: if not novel, please include reference
This is an exciting and significant result.

C Data & methodology: validity of approach, quality of data, quality of presentation
The data is excellent and well characterized. The methodology appears sound.

D Appropriate use of statistics and treatment of uncertainties
The statistics appear appropriate.

E Conclusions: robustness, validity, reliability
The detections are robust, as can be seen from the inset plots and from the significance of the equivalent width.

F Suggested improvements: experiments, data for possible revision
See minor points, below.

G References: appropriate credit to previous work?
See minor points, below.

H Clarity and context: lucidity of abstract/summary, appropriateness of abstract, introduction and conclusions
The abstract and the introductory and concluding paragraphs are clear and lucid.

MINOR POINTS

Line 68. The paper defines a "dense" as a ring where collisions play a significant role. The paper should discuss the range of expected collisions per particle per orbit for its global solution and the observed local parameters.

Line 78. Explicitly say what you are using for a, or say something like, "a is the ring radius for solution 1 of Table 1."

Line 96. If v_esc is the two-body escape velocity of the two ring particles involved in the pairwise impact (rather than the escape velocity from the primary), then say so.

Table 1.
Give the Weywot pole position and error somewhere. Footnote (d) would be a fine place for that.

Fig 1 (e), Supp Fig 4 (d, e, i, l), and in general, the enlarged light curve plots
It would help readability if the dots were printed over the red line.

Fig 2.
Maybe mention that one chord 11 June 2020 is from a spacecraft. Otherwise the line looks odd.

Line 408.
Say where you find or how you calculate stellar diameters. If diameters are from Gaia, it is worth noting that these are listed in DR2, but not EDR3.

Line 413.
Citation to sky plane?

Line 415.
Define Airy scale. Perhaps cite it. It appears that this may be defined in Boissel et al. A&A 561, A144 (2014).
"it can be shown from Roques et al 1987 that the first zero for the ripple amplitude is reached at a distance from the shadow center that we call the Airy scale"

Line 461.
Many readers would like to know estimates of various things that can be estimated once the particle size is known, such as: what is the mass of the ring? What is the optical depth? Is a monolayer or a polylayer? Is it "dense" (e.g., how many collisions per orbit)? This might be a good place to mention those.

Lines 483-484.
The connection between N, radii, local bulk density, and tau is unclear. Is there an unspoken ring thickness?

I do not need to remain anonymous.

Signed, Leslie Young

Author Rebuttals to Initial Comments:

Dear Referee,

We appreciated the suggestions concerning our manuscript entitled "A dense ring around the trans-Neptunian object (50000) Quaoar well outside its Roche Limit". We agree with many of your comments, and we believe they will improve our manuscript. We would like to highlight that the text was revised to comply with Nature editorial policies, so some non-critical discussions were moved to Methods only. All the changes are colored in red in the new manuscript.

Also, follow some comments to your comments:

I do have some suggestions regarding the data analysis and interpretation of the data. None of these should require the authors to do any additional analysis. They are mostly requests to either provide some additional information about their calculations or to revise/clarify some of their arguments.

Specifically with regards to the data analysis, I have three suggestions.

First, as mentioned under point D above, the authors should clarify whether the error bars provided in Table 1 and Supplemental Table 5 correspond to one sigma or three sigma uncertainties.

- Authors' answer: In this paper, the uncertainties are given as 1 sigma, unless it is explicitly described as different from that. Nonetheless, a statement was added in the Tables and Figures captions to ensure that this is clear to the readers.

Second, while the authors provide estimates of the uncertainties on the equivalent width and depth for the positive ring detections, the authors do not provide explicit upper limits on these quantities for most of the occultations with no clear detections (the exception being one case discussed in line 469-475). Having a table of upper limits from the non-detections would be useful for assessing the true range of variations in this ring and comparing it to other rings. For example, the equivalent depth of the F ring ranges between 7 km and 19 km in stellar occultations (see French et al. 2012 Icarus), which is a comparable fractional range to the detections reported in this paper. This would support the comparisons between the two rings made by the authors in line 60 ff. However, if the times the ring was not detected required the equivalent depth of the ring to be much smaller than this range, then it might be worth comparing this ring to Neptune's Adams ring as well. In addition, there appears to be a dip at 80 second in Figure 1f that doesn't look dramatically weaker than the dip seen in Figure 1e, but the caption says that "no definite detection is made.... only an upper limit is derived". Knowing what the upper limit actually was would help the reader to understand why the latter is not a definite detection. I would therefore ask the authors to provide explicit upper limits on the ring equivalent width or depth for the profiles where the ring is not clearly detected. This could be included in one of the supplementary tables.

- Authors' answer: We agree that this information is useful, and we added them to an Extended Data Table. Also, we added a new paragraph in Methods, Section 4, where we better explain how these limits were determined.

Third, while I agree the assumption of uniform opacity rings is sensible for most of the profiles discussed in the paper, the ingress profile from the HiPERCAM occultation appears to show some substructure. In particular, there appears to be a denser core around -148 seconds and maybe a weak gap around -145 seconds. I do not think the authors need to do a detailed analysis of these potential structures in this paper, but the authors should clarify in either Section 3 or 4 of the Methods whether this could affect their model fit parameters, particularly the equivalent width and depth. This would allow the reader to have a more secure understanding of what the parameters given in the tables represent for that particular cut through the ring.

- Authors' answer: We agree that the HiPERCAM detection before the closest approach may indicate some structure within this ring. However, these substructures are mostly within the noise and are yet to be confirmed. Nonetheless, we agree that we should add a disclaimer about this in Methods, Section 3.

I also have some concerns about some aspects of the authors' interpretation of these observations. Specifically, I have issues with the author's statements about the particle sizes in the ring, their discussion of the mechanisms whereby the rings might achieve a high velocity dispersion, and the presented evidence that the ring might be in a spin-orbit resonance for Quaoar. Note that none of these issues reflects a fundamental problem for the paper, instead they are all matters that can be addressed by adjusting some of the text or by providing some more details about their analysis.

First, the authors make some statements about the likely sizes of the particles in the ring that are not appropriate. In particular, the authors say in lines 57-58 that "our numerical integrations show that particles smaller than a few centimeters should escape the ring over a few years timescale due to radiation pressure" However, Methods Section 9 never mentions particles in the few centimeter size range. Instead, this section says that according to Formula 16 "particles smaller than 0.2 cm suffer radial excursions that surpass the ring width" and that only particles smaller than 45 microns collide with Quaoar. They also say that in their simulations particles less that 45 microns "do not survive over a few decades. These statements therefore say that the particles lost to solar radiation pressure are smaller than 100 microns, not a few centimeters. Furthermore, Equation 16 also almost certainly overestimates the eccentricities induced by solar radiation pressure because that particular expression neglects any apsidal precession induced by either Quaoar's J2 or Weywot, which will tend to limit these eccentricities. Also, even if the lifetime of individual sub-centimeter grains is few years, there can still be a steady-state population of such grains released by larger objects (as the authors themselves say on line 65).

Thus the authors have provided no compelling evidence that the ring is "mainly composed of macroscopic (>1 cm particles)" (lines 67-68).

While the above contradiction needs to be rectified, I agree that the ring contains a substantial population of particles larger than a centimeter in radius. I think the issue is that the different statements are creating confusion because they do not clearly distinguish between the particles that produce the signals and the particles that dominate the ring's overall mass. In this context, it is worth considering the F ring. That ring does contain a population of macroscopic particles, but most of the signal in stellar occultations of this ring actually comes from particles in the 1-100 micron size range. As the authors state in lines 63-65, the individual particles in this size range are expected to be relatively short-lived, but there is still a large number of these particles because collisions into and among the larger objects in this ring are constantly releasing new particles into the ring.

I therefore think the authors could address this issue by removing the references to cm-scale particles in the main text, as well as the text saying the ring is "mainly compared of macroscopic particles". Instead they could just say that isolated particles <100 microns will be lost on time scales of years, so there is likely a population of macroscopic particles within this ring.

- Authors' answer: We thank the referee for this thorough revision. We agree that this could cause confusion, and we followed the referee's suggestions.

Second, regarding the interpretation of these data and how a dense ring could exist this far from Quaoar, I agree with the authors that this is most likely because the velocity dispersion of the ring particles is high enough to prevent particles from accreting efficiently. However, the paper does not clearly articulate that there are multiple ways this could happen. After pointing out the need for a large velocity dispersion in line 95ff, the authors make a connection between this dispersion and the particles' coefficient of restitution and focus exclusively on the idea that the collisions in this ring might be sufficiently elastic to avoid aggregation for the next 4 paragraphs. After considering this option, they go on to discuss how external perturbations could also affect this ring, but at this point they also start talking about how the ring is confined. Thus while they mention that resonant perturbations could also potentially increase the velocity dispersion in the ring, the discussion does not make it clear that external perturbations may be a viable alternative to achieving the high velocity dispersions needed to keep the ring material from aggregating.

I therefore suggest that the authors clearly state around line 98 that there are two ways the material in a ring can achieve the high velocity dispersion required to maintain the ring well outside the Roche limit. One being the collisional coefficient of restitution, and the other involving the ring material being perturbed by outside forces. Having both possibilities presented up front would make the following discussions easier for the reader to follow.

- Authors' answer: We agree that stating upfront the two main methods to increase the velocity dispersion will improve the readability of the paper. This was added as suggested.

In addition, I would urge the authors to also consider the possibility that a nearby small moon could also produce perturbations that would allow the ring to persist outside the Roche limit. Numerical simulations of Saturn's F ring already show that gravitational perturbations from the nearby moon Prometheus can generate wide ranges of velocity dispersion at different locations around the ring (see Beurle et al. 2010 ApJL and Sutton and Kusmartsev 2016 EM&P). Much of this work focused on regions of anomalously low velocity dispersion that could favor the formation of small moonlets, but the same simulations also show regions where the velocity distribution gets well above 50 cm/s, which may be enough to disrupt loose aggregates. Thus this also seems like a reasonable explanation for how the ring exists this far from Quaoar.

- Authors' answer: We agree that nearby small moons can also be a possible explanation for the existence of this confined ring well outside the expected Roche limit. This was added together with the previous point. However, no new simulation was done, as we believe this should be the subject of future works, in agreement with the reviewer's comments.

To be clear, I am not saying the authors need to do additional simulations of perturbed rings. I think the simulations of unperturbed ring regions are reasonable for this paper, as they provide good quantitative estimates of what sorts of velocity dispersions are needed to avoid runaway accretion. Simulations of perturbed rings would involve a much broader parameter space that is beyond the scope of this work. I therefore agree with the author's statement in lines 160-170 that this should be the subject of future work.

Finally, the authors present evidence that the ring material may be close to a 1/3 spin-orbit resonance. While this is certainly an intriguing possibility, the authors need to clarify some aspects of their evidence that the resonance is likely to fall close to the ring.

Specifically, in Table 1 the authors state the mass of Quaoar is 1.20+/-0.05 x 10^21 kg. This is actually a new constraint on Quaoar's mass, and is slightly lower and tighter than the previously published number of 1.3-1.5 +/-0.1 x 10^21 kg provided by Fraser et al. 2013. Note that if one uses the central value from this earlier work, the resonance location moves out to 4420 km, thus the case for the resonance's proximity to the ring is considerably stronger with this new estimate of Quaoar's mass. In Methods Section 1 the authors state that this mass comes from a fit done to the same Keck observations as those used by Fraser et al. 2013 along with one occultation from 2019, but they do not explicitly state what caused the central value to shift or the error bars to become tighter. The authors therefore should clarify whether their lower value for Quaoar's mass is due to including the occultation data, or instead has something to do with using the GENOID technique on the Keck observations. Also, the authors should clarify why they included that one particular occultation, and not other occultations like those described in

Braga-Ribas et al. 2013. This information is needed for any future scholar who wants to investigate the role of this resonance in more detail.

- Authors' answer: Indeed, this ring to be located near the 1/3 spin-orbit resonance is an intriguing fact, and that should be elaborated. Genoid is a chi2 minimization algorithm and takes into account the measurement error of the observation. In the case of Quaoar/Weywot, a Keplerian dynamical model is used to model the satellite motion. By fitting only Frazer's data, the algorithm finds a lower mass of Quaoar than that quoted in the Frazer paper, nonetheless, within their 1-sigma error bars. One important point that needs to be clarified is the occultation mentioned here was not an occultation by Quaoar, as the ones published in Braga-Ribas et al., (2013). Instead, it was an occultation by Weywot, observed in Namibia, in 2019, thus constraining Weywot position in its orbit and significantly lengthening the time set (13 years between the first and last observation). Finally, it is important to highlight that the uncertainty on the location of Quaoar 1/3 spin-orbit resonance is strongly affected by our knowledge of Quaoar`s mass and its rotational period. Nonetheless, the uncertainties of these values should be considered and computed and not only one specific value of the distribution. Moreover, as pointed out by the reviewer, if we use Quaoar's mass as proposed by Fraser et al. (2013), the 1/3 resonance location moved to larger values. However, they are still within the 1-sigma error bar compared with the ring location proposed in this work.

Also, in Table 1 the authors give Quaoar's rotation period as 8.8494 hours, but also say the preferred rotation is 17.6788 hours. However, they don't explain why this is the preferred solution in the main body of the paper. In line 555 of the methods they clarify that this solution corresponds to a double-peaked rotation curve. This is a choice, but as far as I am aware there is not yet any independent evidence that favors the 17.6788 hour solution over the 8.8494 hour solution. Thus stating the 17.6788 hour is "preferred" seems to just mean that the author's prefer it because it places the stronger 1/3 spin-orbit resonance near the ring, whereas if the 8.8494 hour solution was correct, the ring would be close to a weaker 1/6 spin orbit resonance. If there is independent evidence that the spin period is the longer one, the authors should point that out explicitly, and if not, the authors should clarify that point. This could be done in a footnote to Table 1, as well as in Methods Section 6.

- Authors' answer: We agree that this point should be clarified. From rotational light curves (Ortiz et al., 2003, A&A, 409, L13), a double-peaked light curve seems to better fit their data. This aligns with the expected rotational light curve of triaxial ellipsoidal objects. This means that the term "preferred" is not for this work alone but in this reference as well. Nonetheless, as the reviewer said, the ~17.7 hours period also places the 1/3 spin-orbit resonance near the observed ring location, and this is a characteristic shared with Chariklo's and Haumea's rings.

My remaining comments are mostly relatively small matters of wording and clarification, which are given below based on line numbers.

Line 18: "All the dense collisional rings observed so far reside inside the Roche limit of their central body...." This sentence may require some adjustment, since there are dense rings (most notably Saturn's outer A ring and Uranus' epsilon ring) that reside close to the Roche Limit itself. I would suggest instead something like "All dense rings are located close enough to their central bodies to be inside the Roche limit where tidal forces would prevent material with reasonable densities from aggregating into a moon."

- Authors' answer: The sentence was modified

Line 23: "thus questioning the validity of this limit" I feel this particular wording is too vague, since it could appear to suggest that the limit has been computed incorrectly, whereas in fact what they argue is that the limit may not always be valid, depending on the physical properties and dynamical environment of the relevant material. I would suggest instead phrasing this to be something like "thus implying that this limit does not always determine where ring material can survive."

- Authors' answer The sentence was modified.

Line 30: "(about half the size of Pluto)" could be "(about half the radius of Pluto)" to be more specific.

- Authors' answer: The sentence was modified.

Line 33: "stellar occultations by this body since 2011." Is the "since 2011" needed? The occultations discussed here all come from after 2018.

- Authors' answer: This sentence was removed.

Line 56: While Bosh et al. 2002 is the best reference for the lack of spectral variations in visible occultations, it may be worth noting that lack of spectral variations in optical depth in Saturn's rings was also reported in the near-infrared by Harbison et al. (2013 Icarus) and in the Ultraviolet by Becker et al. (2018 Icarus).

- Authors' answer: We agree, and these references were added.

Line 58-59: This statement about the composition of the ring being potentially water ice seems out of place here, since the rest of the paragraph is about the particle sizes, not composition. This probably should be deleted since the composition of the rings is not really discussed elsewhere in the paper.

- Authors' answer: This statement where omitted

Line 147: The authors should probably clarify (either here on in Methods Section 6) whether a hemispheric mountain model is compatible with the assumption of the double-peaked rotation curve (which would imply a primarily m=2 anomaly in the gravitational field).

- Authors' answer: The mass anomaly necessary to create a spin-orbit resonance may exist on or inside an oblate body. In the text, it is modeled as a 'mountain' for simplicity to give an idea of what kind of topographic feature is needed to create an effective resonance. However, it may also be an internal feature inside an otherwise oblate, smooth body. To clarify this matter, a sentence was added in Methods, Section 6.

Line 187: "thoroughly" maybe should be deleted, since it still works for most ringed objects.

- Authors' answer: It was removed

Line 210: Reference 10 should probably be to the Icarus article from 2013, not the AAS abstract from 2012.

- Authors' answer: This was corrected

Table 1: The "Date and Station" column for the occultation is confusing and not completely consistent with the rest of the paper.

First of all, there are parenthetical notes "(bef.)" and "(aft.)", but these are not defined anywhere. i think they mean "before" and "after" the body occultation. These terms should be explicitly defined in a footnote.

- Authors' answer: Indeed, these abbreviations mean before and after the closest approach. We added this in a footnote of Table 1.

More importantly, the names for the "Station" do not match the names used in the figures. For example, the Table uses "GTC" for what is called "HiPERCAM" in Figure 1, and "Langersek" for what is called "Algester" in that same figure. I could figure out which was which using Supplementary Table 1, but this did complicate efforts to cross-correlate the numbers in the table with the visual appearance of the occultation dips. The authors therefore need to ensure that the "Stations" given in Table 1 and Supplementary Table 4 match the names provided in the various figures.

- Authors' answer: We agree that this could cause confusion. To improve readability, we standardized the names of the stations in the figures, tables, and in text.

Table 1 also includes the normal optical depth and equivalent width of the ring, but omits opacity and equivalent depth This is not a big deal since all the parameters are given in Supplementary Table 4, but since all four parameters are discussed in the text, they probably should all be included in this table, if possible.

- Authors' answer: We agree, and it was our initial idea to add all these parameters in only one Table, but unfortunately we only had the space to add 4 columns. That is why we created Extended Data Table 2 (former Supplementary Table 4), where all values could be given.

Figure 2: I was initially confused by the 11 June 2020 image because the two cuts were not parallel to each other, which is typically the case for different observations of the same occultation (see for example the 27 August 2021 event). I understand this happened because one of these observations was by the CHEOPS space telescope. However, to clarify the situation for the general reader, I do think it would be worthing briefly mentioned this explicitly in the figure caption.

- Authors' answer: We agree that nonparallel lines in stellar occultation can be odd at first view. That detection was, in fact, the first stellar occultation by a TNO ever predicted and observed by a space telescope; details are in Morgado et al., 2022 (A&A). We highlighted this in this figure's caption.

Figure 3: I found the designations "Model 1-4" a bit opaque on first reading. Since the main difference between these models is how the coefficient of restitution varies with impact speed, I would suggest adding the explicit formula for these models to Figure 3 so that the reader does not have to go to Supplemental Figure 8 to track those down.

- Authors' answer: We agree that Model 1-4 are, in fact, opaque, but we chose to add this in the manner as an alias. Nonetheless, we agree that more details about the models and their complete formulae should be given not only in the caption of Extended Data Figure 4 (former Supplementary Figure 8) but also in Figure 3.

Line 469: "(27 October 2021)" should be "(27 August 2021)"

- Authors' answer: This was Corrected

Line 477: "None of [the prior occultations] provided data with sufficient quality to detect the ring" This statement is a bit surprising, since the S.P, Atacama light curve from Braga-Ribas et al. 2013 looks like it might have been high enough quality to detect the ring, did this occultation simply not cover a wide enough timespan around Quaoar?

- Authors' answer: The San Pedro de Atacama observation on 04 April 2011 covered the region where the ring was expected to be detected. However, unfortunately, it did not have the S/N ratio (and temporal resolution) to detect this ring. To give the values, the single-point radial resolution in the ring plane was 55.2 km, and the Equivalent width (3-sigma) was 0.993 km.

Equation (11) and below. Given epsilon is elsewhere used as the coefficient of restitution, It might be good if the authors would use another symbol besides epsilon_k for the coefficients on the terms in the Hamiltonian. If this is not possible, please include an explicit statement that these coefficients are completely unrelated to the restitution coefficients.

- Authors' answer: We agree that this can cause confusion, so an explicit statement was added to the manuscript.

Line 593-594. In the equation for the Jacobi constant, it might be useful to have a second equality where e^2 is re-written as X^2+Y^2.

- Authors' answer: As suggested, this equality was added.

Line 598: "It is proportional to mu_Q or mu_W". Since mu_W has not been defined yet, and mu_Q is defined in the main text, I would suggest saying instead something like "It is proportional to the normalized mass of the mountain (mu_Q) or Weywot (mu_W)."

- Authors' answer: These parameters were defined in the same section, between the previous lines 585-586.

Supplementary Table 5: If possible, I might suggest adding explicit references to References 44 and 45 for the coefficients.

- Authors' answer: As suggested, the references were added.

Dr. Bruno Eduardo Morgado.

On behalf of all the authors.

Dear Referee,

We appreciated the suggestions concerning our manuscript entitled "A dense ring around the trans-Neptunian object (50000) Quaoar well outside its Roche Limit". We agree with your comments, and we believe they will improve our manuscript. We would like to highlight that the text was revised to comply with Nature editorial policies, so some non-critical discussions were moved to Methods only. All the changes are colored in red in the new manuscript.

Also, follow some comments to your comments:

Line 68. The paper defines a "dense" as a ring where collisions play a significant role. The paper should discuss the range of expected collisions per particle per orbit for its global solution and the observed local parameters.

- Authors' answer: In a first approximation, the rate of collisions per orbit per particle can be computed as a function of the optical depth of the ring (See, for instance, Hämeen-Anttila & Salo, 1993, Earth, Moon and Planets 62, 1, 47-84). In our specific case, this value would be mostly between 0.6 and 10.0 collisions per particle per orbit. We added these values to our manuscript in Methods, Section 2.

Line 78. Explicitly say what you are using for a, or say something like, "a is the ring radius for solution 1 of Table 1."

- Authors' answer: At this level of estimation, both Solutions (ring at 4,097 or 4,148 km) will result in a Roche critical density of around 30 kg m^{-3}.

Line 96. If v_esc is the two-body escape velocity of the two ring particles involved in the pairwise impact (rather than the escape velocity from the primary), then say so.

- Authors' answer: In fact, it is the escape velocity between both particles and not from the primary. We clarify this in the manuscript.

Table 1. Give the Weywot pole position and error somewhere. Footnote (d) would be a fine place for that.

- Authors' answer: As recommended, Weywot's pole position value was given in Footnote (d) of Table 1.

Fig 1 (e), Supp Fig 4 (d, e, i, l), and in general, the enlarged light curve plots

It would help readability if the dots were printed over the red line.

- Authors' answer: We tested, and printing the dots over the red line worsened the readability of the model for most figures because the figure became crowded with points.

Fig 2. Maybe mention that one chord 11 June 2020 is from a spacecraft. Otherwise the line looks odd.

- Authors' answer: We agree that nonparallel lines in stellar occultation can be odd at first view. That detection was, in fact, the first stellar occultation by a TNO ever predicted and observed by a space telescope; details are in Morgado et al., 2022 (A&A). We highlighted this in this figure's caption.

Line 408. Say where you find or how you calculate stellar diameters. If diameters are from Gaia, it is worth noting that these are listed in DR2, but not EDR3.

- Authors' answer: Indeed, the stellar diameter was obtained from Gaia DR2. We added this information in the manuscript.

Line 413. Citation to sky plane?

- Authors' answer: A reference was added.

Line 415. Define Airy scale. Perhaps cite it. It appears that this may be defined in Boissel et al. A&A 561, A144 (2014).

"it can be shown from Roques et al 1987 that the first zero for the ripple amplitude is reached at a distance from the shadow center that we call the Airy scale"

- Authors' answer: The reference was added in the text.

Line 461. Many readers would like to know estimates of various things that can be estimated once the particle size is known, such as: what is the mass of the ring? What is the optical depth? Is a monolayer or a polylayer? Is it "dense" (e.g., how many collisions per orbit)? This might be a good place to mention those.

- Authors' answer: We agree that we can estimate and include comments about these parameters. We added them in the Methods, Section 2.

Lines 483-484. The connection between N, radii, local bulk density, and tau is unclear. Is there an unspoken ring thickness?

- Authors' answer: There is no unspoken ring thickness in the paper.

Dr. Bruno Eduardo Morgado.

On behalf of all the authors.

Reviewer Reports on the First Revision:

Referees' comments:

Referee #1 (Remarks to the Author):

A: Summary of the key results

The revised manuscript "A dense ring around the trans-Neptunian object (50000) Quaoar well outside its Roche Limit by B.E. Morgado et al. describes evidence from a series of stellar occultations that there is a ring of material around Quaoar. This ring is particular interesting because it is located so far from Quaoar that it should be outside the classical Roche limit, implying that some dynamical process is preventing this material from aggregating into a moon.

B: Originality and significance: if not novel, please include reference

These findings are novel and should be interest to a broad interest to the planetary science community. While planetary rings have been previously found around small bodies, this very distant ring of material is unexpected and suggests that we need to re-evaluate how quickly orbiting material will coagulate into larger bodies.

C: Data & methodology: validity of approach, quality of data, quality of presentation

The evidence for this new ring also appears to be solid. Stellar occultations are extremely sensitive to these sorts of narrow rings, and this team has extensive experience in observing these events and processing the resulting data. The relevant data are clearly illustrated and described.

D: Appropriate use of statistics and treatment of uncertainties

Overall, the treatment of uncertainties is reasonable. Specifically, I thank the authors for including the limits on ring detections from other occultations. I would just ask that the authors also include the upper limits on any ring signal from the "after" cuts for the Reedy Creek, Algester and Samford Valley occultations. Since the paper shows those data in the various figures, having an explicit upper limit on the equivalent width from those observations would be extremely useful.

E: Conclusions: robustness, validity, reliability

The conclusion that Quaoar has a ring of orbital debris appears to be very robust, given the presented evidence. This ring is also reliably well outside the Roche limit. The idea that this is because the velocity dispersion of the ring particles is high enough to inhibit ring material from accreting into moons is also a reasonable explanation for this finding.

F and G: Suggested improvements: experiments, data for possible revision

Most of my suggestions are small matters of wording, that can be implemented at the discretion of the authors and editors. I have only two suggestions that might require additional calculations.

First, as mentioned under point D above, I would urge the authors to please include explicit limits for any ring signal from the "after" cuts for the Reedy Creek, Algester and Samford Valley occultations in

Supplementary Table 3.

Second, if it can be done relatively easily, the authors could consider including the longitudes of the various occultation cuts through the rings in an inertial coordinate system (for example, relative to the ring's longitude of ascending node on J2000). These numbers would be useful for future efforts to determine the importance of resonances with Quaoar's spin period and Weywot's orbital period. The author's estimates of these periods have sufficiently small errors that it should be possible to examine whether the opacity variations are consistent with material moving at the correct rate to be in either of these resonances, as was done for the arcs in Neptune's rings (e.g. Nicholson et al. 1990). Thus, if the authors have this information readily available, including it in Extended Data Tables 2 and 3 would benefit future efforts to understand this ring. However, if the authors feel this would be too difficult to do at the present time, or is more appropriate for a follow-up analysis with more occultation data, that is fine and should not affect the publication of this manuscript. Even in this case, it would still be worth mentioning the possibility of tracking the motion of dense arcs over time as another way to test models of resonant confinement near the end of the paragraph covering lines 139-147.

Remaining suggestions with regards to the text of the manuscript are provided below with specific line numbers.

Line 21-22: "with a dense and narrow arc discovered from Australia on 27 Aug 2021" The implications of the "discovered" here is not clear because the rest of the paper does not particularly focus on those particular observations, and does not indicate whether this particular event was where the ring as a whole was first noticed. I would therefore suggest this clause be revised to either say "with a particularly dense and narrow arc observed from Australia on 27 Aug 2021" or "that was first identified when a particularly dense and narrow arc was observed from Australia on 27 Aug 2021", whichever is appropriate.

Line 30: Consider adding the qualifier "for small bodies," to the end of this sentence since 1/3 spin-orbit resonances are not relevant for the rings of the giant planets.

Line 42: Since p and tau are derived from a model that assumes uniform ring opacity, the text should be adjusted to say something like "the _mean_ normal opacity p and the _mean_ normal optical depth tau" or "the _average_effective_ normal opacity p and the _average_effective_ normal optical depth tau"

Line 48-49: "implying that this core contains mainly particles larger than 10 microns". This statement is too strong, since Bosh et al. 2002 only concludes that the F-ring "contains a population of particles larger in size than ~ 10 um" and both Bosh et al. 2002 and Becker et al. 2018 both consider size distributions that extend below 10 um. Furthermore, Vahidinia et al. 2011 (Icarus) and Hedman et al. 2011 (Icarus) both found evidence that the particle size distribution cannot extend too far above 30 microns, which is consistent with the particle-size cut-off the authors predict for this ring in Methods Section 9. Thus I feel it would be more appropriate to say: "implying that this core contains a substantial population of particles larger than around 10 microns".

Line 52ff: While I agree that the F ring is one sensible point of comparison for this irregular ring, the authors should also mention the arcs in Neptune's Adams Ring. This ring also shows substantial longitudinal variations in optical depth and is located outside the orbits of several of Neptune's moons and so may also require the same processes to prevent material from accreting into moons.

Line 62: "They are" could be "These tenuous rings are"

Line 64-66: I would suggest revising the text to say "should have a density greater than the Roche critical density .... in order to avoid being disrupted by tidal forces"

Table 1, Footnote a: add explicit reference to Methods section at the end of this footnote

Line 438: The range of optical depths down to 0.004 corresponds to the lowest optical depth limit in Extended Data Table 3 and Methods Section 4, so those should be referenced here.

Line 470-471: Again, I would suggest re-phrasing this to say: ""indicates that Quaoar's ring contains a substantial population of particles larger than around 10 microns".

Line 474: "besides the one" should be "besides those"

Line 485: Please clarify which entry in Extended data table 3 corresponds to "W. Hanna's data set" by adding "from Yazz".

Line 701: "This force causes no secular effect in the semi-major axis of the particles" This statement is not quite true, since some regard Poynting-Robertson drag as part of solar radiation pressure. I would therefore suggest adjusting the wording slightly to say "To first order, this force causes no secular effect...."

G. References: appropriate credit to previous work?

References are appropriate, but if possible, I would suggesting adding references to Beurle et al. 2010 and Sutton and Kurmartsev 2016 at the appropriate locations, perhaps in the Methods section.

H. Clarity and context: lucidity of abstract/summary, appropriateness of abstract, introduction and conclusions

The paper puts these results in context very well, the abstract and introduction are both clear and lucid.

Referee #2 (Remarks to the Author):

Review of Morgado et al., "A dense ring around the trans-Neptunian object (50000) Quaoar well outside its Roche Limit."

A Summary of the key results
This brings the number small bodies with rings from two to three.
Each are remarkable. This one is remarkable largely for its location.

B Originality and significance: if not novel, please include reference
This is an exciting and significant result.

C Data & methodology: validity of approach, quality of data, quality of presentation
The data is excellent and well characterized. The methodology appears sound.

D Appropriate use of statistics and treatment of uncertainties
The statistics appear appropriate.

E Conclusions: robustness, validity, reliability
The detections are robust, as can be seen from the inset plots and from the significance of the equivalent width.

F Suggested improvements: experiments, data for possible revision
I have no suggested improvements.

G References: appropriate credit to previous work?
Yes.

H Clarity and context: lucidity of abstract/summary, appropriateness of abstract, introduction and conclusions
The abstract and the introductory and concluding paragraphs are clear and lucid.

General:

This is an exciting and significant result. The authors addressed my previous points, all of which were minor. I recommend publication with no further revision. I do not need to see this again.

Author Rebuttals to First Revision:

Dear Referee,

We appreciated the suggestions concerning our manuscript entitled “A dense ring of the trans-Neptunian object Quaoar outside its Roche Limit". We agree with your comments, and we believe they improved our manuscript. All the changes are colored in red in the new manuscript.

Also, follow some comments to your suggestions:

First, as mentioned under point D above, I would urge the authors to please include explicit limits for any ring signal from the "after" cuts for the Reedy Creek, Algester and Samford Valley occultations in Supplementary Table 3.

- **Authors’ answer:** We agree that these values are important, and they were added in Supplementary Table 3.

Second, if it can be done relatively easily, the authors could consider including the longitudes of the various occultation cuts through the rings in an inertial coordinate system (for example, relative to the ring's longitude of ascending node on J2000). These numbers would be useful for future efforts to determine the importance of resonances with Quaoar's spin period and Weywot's orbital period. The author's estimates of these periods have sufficiently small errors that it should be possible to examine whether the opacity variations are consistent with material moving at the correct rate to be in either of these resonances, as was done for the arcs in Neptune's rings (e.g. Nicholson et al. 1990). Thus, if the authors have this information readily available, including it in Extended Data Tables 2 and 3 would benefit future efforts to understand this ring. However, if the authors feel this would be too difficult to do at the present time, or is more appropriate for a follow-up analysis with more occultation data, that is fine and should not affect the publication of this manuscript. Even in this case, it would still be worth mentioning the possibility of tracking the motion of dense arcs over time as another way to test models of resonant confinement near the end of the paragraph covering lines 139-147.

- **Authors’ answer:** We agree that these values are important. However, we believe that this analysis is outside the scope of this project and it can be better explored in future projects. As suggested, a sentence was added to highlight this possibility.

Line 21-22: "with a dense and narrow arc discovered from Australia on 27 Aug 2021" The implications of the "discovered" here is not clear because the rest of the paper does not particularly focus on those particular observations, and does not indicate whether this particular event was where the ring as a whole was first noticed. I would therefore suggest this clause be revised to either say "with a particularly dense and narrow arc observed from Australia on 27 Aug 2021" or "that was first identified when a particularly dense and narrow arc was observed from Australia on 27 Aug 2021", whichever is appropriate.

- **Authors’ answer:** The sentence was modified following the editor’s suggestion.

Line 30: Consider adding the qualifier "for small bodies," to the end of this sentence since 1/3 spin-orbit resonances are not relevant for the rings of the giant planets.

- **Authors' answer:** The sentence was modified.

Line 42: Since p and tau are derived from a model that assumes uniform ring opacity, the text should be adjusted to say something like "the _mean_ normal opacity p and the _mean_ normal optical depth tau" or "the _average_effective_ normal opacity p and the _average_effective_ normal optical depth tau"

- **Authors' answer:** The sentence was modified.

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- **Authors' answer:** The sentence was modified.

Line 52ff: While I agree that the F ring is one sensible point of comparison for this irregular ring, the authors should also mention the arcs in Neptune's Adams Ring. This ring also shows substantial longitudinal variations in optical depth and is located outside the orbits of several of Neptune's moons and so may also require the same processes to prevent material from accreting into moons.

- **Authors' answer:** This comparison was added.

Line 62: "They are" could be "These tenuous rings are"

- **Authors' answer:** The sentence was modified.

Line 64-66: I would suggest revising the text to say "should have a density greater than the Roche critical density .... in order to avoid being disrupted by tidal forces"

- **Authors' answer:** The sentence was modified.

Table 1, Footnote a: add explicit reference to Methods section at the end of this footnote

- **Authors' answer:** A reference to Methods was added.

Line 438: The range of optical depths down to 0.004 corresponds to the lowest optical depth limit in Extended Data Table 3 and Methods Section 4, so those should be referenced here.

- **Authors' answer:** We added a reference to the Extended Data Table 3.

Line 470-471: Again, I would suggest re-phrasing this to say: ""indicates that Quaoar's ring contains a substantial population of particles larger than around 10 microns".

- **Authors' answer:** The sentence was modified.

Line 474: "besides the one" should be "besides those"

- **Authors' answer:** The sentence was modified.

Line 485: Please clarify which entry in Extended data table 3 corresponds to "W. Hanna's data set" by adding "from Yazz".

- **Authors' answer:** The sentence was modified.

Line 701: "This force causes no secular effect in the semi-major axis of the particles" This statement is not quite true, since some regard Poynting-Robertson drag as part of solar radiation pressure. I would therefore suggest adjusting the wording slightly to say "To first order, this force causes no secular effect...."

- **Authors' answer:** The sentence was modified.

References are appropriate, but if possible, I would suggesting adding references to Beurle et al. 2010 and Sutton and Kurmartsev 2016 at the appropriate locations, perhaps in the Methods section.

- **Authors' answer:** We added a reference to Beurle et al. 2010 in our manuscript. However, we do not fully understand the methods in Sutton and Kusmartsev (2016), so we are not comfortable adding them as a reference in this paper.

Dr. Bruno Eduardo Morgado.

On behalf of all the authors.