PEER REVIEW REPORT

Planetary Research

This peer review report contains the editor decisions, reviews, and author rebuttals for each round of peer review. The peer review rounds are ordered chronologically, and the final acceptance decision is not included.

The published manuscript can be found on the journal website: https://planetary-research.org.

TITLE

Elastic-plastic flexure and heat flux on icy moons

AUTHORS

A. Broquet

DOI

https://doi.org/10.53480/d2r6-gh41

EDITOR DECISION
round 1

Dear Adrien Broquet,

I would like to thank you for submitting your manuscript "Elastic-plastic flexure and heat flux on icy moons" (#238) to Planetary Research. I have now received the reviews and associate editor assessment of your submission, which are included at the bottom of this email.

The reviewers find the study valuable and consider the elastic-plastic flexure model to be a significant contribution to understanding the thermal and mechanical evolution of icy moon lithospheres.

Before the manuscript can proceed further, several points need to be addressed. First, the discussion and conclusions should be revised to avoid the manuscript appearing to dismiss elastic flexure models too broadly. Your own results show that elastic and elastic-plastic solutions can agree well in low-curvature cases, and this limitation should be stated more clearly.

Second, the discussion should more thoroughly explain the limitations of the elastic-plastic approach as a static approximation.

Third, the recommendation to use maximum curvature when relating elastic thickness to the yield strength envelope requires a better justification. At present, this conclusion appears to rely on a limited synthetic example, and its robustness under varying conditions should be examined in greater detail.

Finally, the Ariel case study and the proposed heat-flux scaling argument require clearer explanation and more robust support, particularly in instances where mechanical thickness is used to reinterpret previous estimates. The approach to uncertainty in fitting topographic profiles should be enhanced, and all typographical issues and missing reference details should be addressed.

In addition to the reviewers' comments, I encourage you to present the exact numerical values used more transparently, ideally in a summary table or appendix. Providing the final input parameters for the principal synthetic and observational cases would enhance the clarity of the methodology and facilitate the reuse of results in future studies.

Please provide us with a point-by-point response to all comments and identify what changes you have made to the revised manuscript. In addition to the rebuttal, we ask that you also provide a document that tracks all changes that have been made to your manuscript since its initial submission.

When you have completed your revisions, you can upload the revised manuscript, your response and a tracked changes document from your submission dashboard.

If you have any questions about the recommendations in your review, or if you are unsure how to respond or proceed, please do not hesitate to contact me from your submission dashboard.

Sincerely,

Marie Běhounková
Editor, Planetary Research

Associate editor recommendation

The manuscript offers a valuable extension of lithospheric flexure modeling by incorporating elastic–plastic rheology, with clear potential to improve estimates of thermal structure and heat flux on icy bodies. However, some issues should be addressed before publication, as suggested by the two reviewers. The practical application would benefit from a more rigorous treatment of uncertainties in topography-derived geometric parameters, ideally through quantitative analysis. The conclusions should also be more carefully framed, as elastic models remain valid in low-curvature regimes. In addition, the manuscript would benefit from a clearer discussion of limitations, particularly the neglect of time-dependent flexural evolution, and from a more cautious presentation of the maximum-curvature criterion and the Ariel interpretation. Overall, the study is promising but requires clearer framing and a more balanced interpretation before acceptance.

The following comments were received from the reviewers.

Reviewer 1:
Recommendation: Revisions Required With Review

All pre-existing data used in the manuscript are publicly available and sufficient information is provided so that they can be found and accessed.

Yes

All significant data products generated as part of the study have been uploaded to a public repository (or will be at the time of acceptance) and are cited using a persistent identifier (such as a DOI).

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All pre-existing software used as part of the study are acknowledged, and sufficient information is provided to find, access, and understand the software. If the software is restricted, the conditions under which it can be accessed are publicly available.

Yes

If you answered no to any of the above questions, please provide supporting information.

I am uploading an annotated version of the manuscript.

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Comments for the authors

This paper by Broquet develops the more realistic elastic-plastic flexure models to ascertain heat flux on icy moons. The paper claims, and rightly so that a lot of papers in the past have mostly considered the elastic nature of the icy-shell and that consideration of the plastic nature of the icy shell can provide more realistic estimates of heat flow. The elastic-plastic flexure model is applied to Ganymede and Ariel and lower heat flow values than in the past is constrained. I believe the development of the elastic-plastic model would be valuable contribution in the field and I only have only a few concerns about the paper below. The paper is also well written and I only noticed few typos/grammatical issues that I list below.

1. I think the paper may be overstating the case again the elastic approach in places. The paper argues that the elastic-plastic model "should thus be preferred over the classical broken elastic plate model" and that previous heat fluxes were "largely overestimated" because of elasticity assumptions and Deborah number methods. However, the manuscript itself also shows that elastic and elastic-plastic solutions can agree well in lower-curvature cases and that elastic models remain adequate below a saturation threshold. Unless I am misunderstanding something here, the conclusions should be reframed more carefully so the paper does not read as a complete and total blanket dismissal of elastic flexure methods.

2. One of my other main concerns is that, although I recognize that elastic-plastic flexure models are an improvement over strictly elastic models, previous work has already shown the importance of temporal evolution of flexure. If the flexural signals we observe and attempt to model are inherently non-static (and not representative of flexure from the time of the loading), then I wonder whether the shift from a purely elastic model to an elastic-plastic one is, in a deeper sense, still missing the essential physics. The elastic-plastic estimates may well be more "realistic" than purely elastic ones, but if we already agree that the true system evolves through time, then this is still not really the correct framework. In that case, does adopting an elastic-plastic model instead of a purely elastic one actually provide physically meaningful information, or does it simply offer a somewhat improved but nevertheless a flawed approximation via a fundamentally inadequate approach? I know the paper sorta acknowledges this in the introduction, but some guidance in the discussion about this would be beneficial.

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4. The discussion around Ariel, including the argument that the heat flux should be divided by about three because the mechanical thickness is two to three times larger, needs more rigorous support or at least clearer framing as an order-of-magnitude interpretation rather than a strict correction. I re-read this section multiple times and am still lost with the chain of thought here.

Line35: "allowing to test various parameters at multiple locations." Sounds vague to me.
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Comments for the authors

This is a review by William Moore of the manuscript entitled “Elastic-plastic exure and heat ux on icy moons” submitted by A. Broquet to Planetary Research. My name may be associated with this review.

The manuscript is a worthwhile application of failure envelope flexure theory to the problem of inferring thermal structure from lithospheric flexure. In particular, the comparison with the broken plate model reveals a useful technique for identifying the relevant point at which curvature should be estimated. There are some issues with the practical application of the approach, however, that, if addressed, would significantly improve the paper. Additionally, some minor suggestions are made below.

The primary problem of practical application is that, as the Ariel example in figure 5 demonstrates, there is considerable uncertainty in deriving the basic geometric factors of plate end point, reference elevation, and zero-crossing from observed topography (one can also add a regional slope parameter). This could be addressed by adding an offset (in a monte-carlo fashion) in the horizontal and vertical position of the elastic model in the fits in figure 2. The resulting spread in best fit models would give an estimate of the range of uncertainty in retrieving elastic plate parameters from actual topographic data with noise. This would increase the value of this article to the field.

Specific items:

Line 121: It isn't true that upward and downward deflection are symmetric in the plastic case, althoguh the yield envelope in fig 1 a is fairly symmetric, fig 1b. shows a difference in the slope of the elastic-plastic curve for positive vs. negative curvature. Is the point here that the elastic-plastic plate has a larger mechanical thickness than the elastic solution's thickness regardless of direction?

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AUTHOR REBUTTAL
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Finally, the Ariel case study and the proposed heat-flux scaling argument require clearer explanation and more robust support, particularly in instances where mechanical thickness is used to reinterpret previous estimates. The approach to uncertainty in fitting topographic profiles should be enhanced, and all typographical issues and missing reference details should be addressed.

In addition to the reviewers' comments, I encourage you to present the exact numerical values used more transparently, ideally in a summary table or appendix. Providing the final input parameters for the principal synthetic and observational cases would enhance the clarity of the methodology and facilitate the reuse of results in future studies.

Marie Běhounková
Editor, Planetary Research

I thank the reviewers, Associate Editor and Editor for their valuable comments. All are addressed below. For clarity, my replies to comments are written in blue.

Following the suggestions, I have revised the text to better emphasize the reliability of the elastic models at low curvatures and have added a section discussing limits of the static assumption (Section 4.3). I also now present a Figure (now Figure S1) showing that the maximum curvature argument remains valid over a wide range of heat flux and flexural parameter w0.

In the case of Ariel, I have tried to improve the discussion explaining why previous work that used a Deborah number approach might have overestimated the heat flux.

I also have added a Table (Table S1) summarizing input parameters for the yield strength envelope and heat flux estimates.

Associate editor recommendation

The manuscript offers a valuable extension of lithospheric flexure modeling by incorporating elastic–plastic rheology, with clear potential to improve estimates of thermal structure and heat flux on icy bodies. However, some issues should be addressed before publication, as suggested by the two reviewers. The practical application would benefit from a more rigorous treatment of uncertainties in topography-derived geometric parameters, ideally through quantitative analysis. The conclusions should also be more carefully framed, as elastic models remain valid in low-curvature regimes. In addition, the manuscript would benefit from a clearer discussion of limitations, particularly the neglect of time-dependent flexural evolution, and from a more cautious presentation of the maximum-curvature criterion and the Ariel interpretation. Overall, the study is promising but requires clearer framing and a more balanced interpretation before acceptance.

Many thanks for the positive feedback and comments. This paper was initially written to demonstrate that purely elastic models sometimes predict unrealistic oscillations that can bias the estimated elastic thickness and heat flux. For that reason, I focused on comparing theoretical models rather than trying to fit observations to derive heat flux estimates.

Nevertheless, and to address the comments from the Associate Editor, I have now added a more rigorous estimate of the heat flux at Ariel and Ganymede by considering the variability in the observed topography profiles as well as the uncertain in the stereo-dems and exploring a wider parameter space of heat flux and central flexure w0. I note that previous work did not account for this and only provided a best-fit. As noted in the text, that is because the uncertainty in the input parameters (grain size, strain rate) affect as strongly the estimated heat flux as uncertainties in the topography.

I have also added / edited several sentences to emphasize that elastic models are adequate in low curvature regimes (please see my reply to Comment 1 by Reviewer 1) and added a discussion section on the static assumption (section 4.3).

Reviewer 1:

This paper by Broquet develops the more realistic elastic-plastic flexure models to ascertain heat flux on icy moons. The paper claims, and rightly so that a lot of papers in the past have mostly considered the elastic nature of the icy-shell and that consideration of the plastic nature of the icy shell can provide more realistic estimates of heat flow. The elastic-plastic flexure model is applied to Ganymede and Ariel and lower heat flow values than in the past is constrained. I believe the development of the elastic-plastic model would be valuable contribution in the field and I only have only a few concerns about the paper below. The paper is also well written and I only noticed few typos/grammatical issues that I list below.

I greatly appreciate the positive feedback and comments, which are all addressed below.

1. I think the paper may be overstating the case again the elastic approach in places. The paper argues that the elastic-plastic model "should thus be preferred over the classical broken elastic plate model" and that previous heat fluxes were "largely overestimated" because of elasticity assumptions and Deborah number methods. However, the manuscript itself also shows that elastic and elastic-plastic solutions can agree well in lower-curvature cases and that elastic models remain adequate below a saturation threshold. Unless I am misunderstanding something here, the conclusions should be reframed more carefully so the paper does not read as a complete and total blanket dismissal of elastic flexure methods.

The reviewer is perfectly right, this was stated too strongly. I have adjusted the text as follows:

Abstract line 20: "*We show that, **under high plate curvature (> 10^{-5} m^{-1})**, purely elastic models predict unrealistic oscillations near and in the flexural bulge region, which can bias the estimated elastic thickness and heat flux.*"

Added a new sentence
Line 385: "*Nevertheless, we find that elastic and elastic-plastic solutions can agree well in low plate curvature geologic settings, such that elastic models remain adequate below a specific plate rheology dependent bending-moment saturation threshold*."

line 416: "*We find the heat flux**, in these high curvature geologic settings,** to have been largely overestimated, by a factor of two and more*".

Nevertheless, I would still argue that elastic-plastic models should be preferred over purely elastic solutions, particularly because the elastic-plastic model does not need to consider an elastic thickness to estimate the heat flux.

2. One of my other main concerns is that, although I recognize that elastic-plastic flexure models are an improvement over strictly elastic models, previous work has already shown the importance of temporal evolution of flexure. If the flexural signals we observe and attempt to model are inherently non-static (and not representative of flexure from the time of the loading), then I wonder whether the shift from a purely elastic model to an elastic-plastic one is, in a deeper sense, still missing the essential physics. The elastic-plastic estimates may well be more "realistic" than purely elastic ones, but if we already agree that the true system evolves through time, then this is still not really the correct framework. In that case, does adopting an elastic-plastic model instead of a purely elastic one actually provide physically meaningful information,

or does it simply offer a somewhat improved but nevertheless a flawed approximation via a fundamentally inadequate approach? I know the paper sorta acknowledges this in the introduction, but some guidance in the discussion about this would be beneficial.

That is a great point that indeed deserved more discussion. The static assumption is an oversimplification of reality and I fully agree that an elasto-visco-plastic model, wherein time is accounted for, would provide a better representation of the ice-shell behavior. However, it necessarily requires adding a viscosity/time component to the model, which, as far as I am aware, is fully unknown. Even with more data this fundamentally unconstrained problem would remain challenging to address, and this is easily shown by simply looking at other planets (Venus, Mars, Mercury) where elastic flexure is typically assumed.

In addition, I would like to stress that elastic-plastic flexure can be considered a good representation of the flexural response providing a sufficient time has elapsed since the deformation. The time required depends on the interior viscosity and can be approximated as the Maxwell time or using a full Love number calculation approach using some input interior models. However, as discussed above, such time is challenging to constrain.

To reflect this discussion, I have added a new section (4.3) discussing the static assumption:

"*Our ice-shell flexure models make the important assumption that the observed deformation is static and/or at steady state, which is only valid if the time elapsed since the deformation started is greater than the time required for viscous adjustments (e.g. Peltier 1974). Icy moons are dynamic worlds (e.g. McKinnon 1998; Dombard and McKinnon 2006; Van Hoolst et al. 2024) and it is likely that some of the observed surface deformations are still ongoing. However, considering the ice-shell transient viscoelastic response would require knowledge on the age of the observed deformation features, which is currently poorly constrained (e.g. Cadek et al. 2017). Nevertheless, it is important to note that modelling the ice-shell transient response with a static model in which deformations are instantaneous would generally lead to an overestimation of the ice-shell yield strength (and underestimation of the heat flux). Indeed, if the deformation is still ongoing, only a fraction of the final deformation would be observed and this would make a static model misinterpret the shell as being stronger and colder than it is. Any subsequent viscoelastic relaxation processes unrelated to the loading event would also tend to alter and reduce the observed deformation wavelengths and lead to an overestimation of the ice-shell yield strength. We note that such problems related to estimates of paleo elastic flexure and heat flux are common in geophysical modelling of the terrestrial planets (e.g. Broquet and Wieczorek 2019; Smrekar, Ostberg, and O'Rourke 2022).*

Importantly, as the predicted heat fluxes for Ganymede are far above the estimated radiogenic heat fluxes of 40 mW m^{-2} (e.g. Bland, Showman, and Tobie 2009), it is likely that the observed deformations have been recorded during a time discrete major tidal heating event. Such deformations are thus likely to have been frozen in the ice shell and be preserved to present-day."

3. One of the other main results from this paper is that the maximum curvature of the synthetic best-fit broken plate should be used when linking an elastic thickness estimate to a YSE. This is further supported by the presented Europa example. However, I am not sure how to interpret this result in a broader context. The manuscript presently demonstrates this point with one main YSE setup and a limited synthetic test. But, how robust is this choice to different thermal gradients, strain rates, grain sizes, shell thicknesses, or loading geometries? As written, the paper makes a broad methodological recommendation from a small synthetic example/demonstration. Is that adequate? Can future work just take this recommendation at face value?

Thank you for raising this issue. I have now added some text on this together with a new Figure S1 demonstrating that taking the maximum curvature is adequate for a wide range of heat fluxes and central deflection w0, which together cover a wide range of plate rheology and loading geometry.

Line 176:
"*We have also explored a wider parameter space considering positive and negative w0 ranging from -4 km to 4 km as well as heat fluxes of 20 to 300 mW m^{-2} (Figure S1). In all tested scenarios, we look for the input curvature that allows for a best-fit between the elastic model (which depends on an elastic thickness and curvature) and the elastic-plastic solution (which depends on a heat flux). The best-fit curvature is then compared to the maximum curvature of the synthetic elastic model, as was done above. This analysis shows that for a wide range of heat fluxes (thus yield strength), as well as central deflection (thus curvature), selecting the maximum curvature of the synthetic elastic profile is always a good approach to match elastic-plastic and elastic models and therefore to estimate the heat flux from an elastic thickness.* "

4. The discussion around Ariel, including the argument that the heat flux should be divided by about three because the mechanical thickness is two to three times larger, needs more rigorous support or at least clearer framing as an order-of-magnitude interpretation rather than a strict correction. I re-read this section multiple times and am still lost with the chain of thought here.

I tried to provide some guidance to explain why previous work that used a Deborah number approach might have overestimated the heat flux. I have made some modifications to this discussion section and hope it makes it clearer.

Line 296:
"*As discussed above, this inconsistency may stem from the assumption of plate elasticity, as the estimated central deflection and plate curvatures are large (~ 10^{-5}–10^{-6} m^{-1}, Figure S2). Another possibility is related to the Deborah number approach used to estimate the temperature at the base of the elastic lithosphere and the associated heat flux (Nimmo+2002). The assumed Deborah number of 0.01 represents the materials' behaviour at the crossover depth between the elastic lithosphere and viscous asthenosphere. However, simply translating elastic thickness to elastic depth is an idealized view that does not consider the lithosphere as an elasto-visco-plastic*

medium. Elasticity within a lithosphere typically occurs within an "elastic-core" located near the midpoint of the plate (McNutt 1984; Burov 2010). Under low curvatures, the entire plate behaves nearly elastically (mechanical thickness equals lithosphere thickness) and the thickness of the lithosphere can be used to estimate the base of the elastic lithosphere. In high curvature regimes, the elastic core is confined to the middle part of the plate, the top of the elastic lithosphere does not reach the surface and thus the base of the elastic lithosphere is systematically deeper than its thickness. As a result, the depth of the base of the elastic lithosphere is underestimated using the Deborah number approach and the heat flux is overestimated. This behavior is conceptually observed in the misfit plots, where the Deborah number predicted heat flux increases away from the elastic-plastic fit as the central flexure w0 (and hence plate curvature) increases (Figures 5 and S2). For small w0 (<3 km) and heat flux (<30 mW m-2), we find curvatures of less than 10^{-6} m^{-1} (Figure S2) and both approaches are found to provide comparable fits to observations."

Line35: "allowing to test various parameters at multiple locations." Sounds vague to me.
Indeed, that was unclear. Changed to "allowing to explore a wide parameter range at multiple locations"

Line 36: development of can likely be deleted here
Changed as suggested

Line 48: One critical issue during… suggest rewording to "One critical issue with"
Changed as suggested

Line 184: "which values of ~ 4 km" Not sure what is meant here.
This typo has been corrected as "with values reaching ~4 km"

Line 387: Reference missing information
Corrected.

Reviewer 2:
This is a review by William Moore of the manuscript entitled "Elastic-plastic exure and heat ux on icy moons" submitted by A. Broquet to Planetary Research. My name may be associated with this review.

The manuscript is a worthwhile application of failure envelope flexure theory to the problem of inferring thermal structure from lithospheric flexure. In particular, the comparison with the broken plate model reveals a useful technique for identifying the relevant point at which curvature should be estimated. There are some issues with the practical application of the approach, however, that, if addressed, would significantly improve the paper. Additionally, some minor suggestions are made below.

Many thanks for this valuable feedback. All concerns are addressed in detail below.

The primary problem of practical application is that, as the Ariel example in figure 5 demonstrates, there is considerable uncertainty in deriving the basic geometric factors of plate end point, reference elevation, and zero-crossing from observed topography (one can also add a regional slope parameter). This could be addressed by adding an offset (in a monte-carlo fashion) in the horizontal and vertical position of the elastic model in the fits in figure 2. The resulting spread in best fit models would give an estimate of the range of uncertainty in retrieving elastic plate parameters from actual topographic data with noise. This would increase the value of this article to the field.

The reviewer is entirely correct here. There is substantial uncertainty in the observed topographic profile and in the way one should fit the model to observation.

As discussed above, I have conducted a more in-depth parameter-space investigation by varying the heat flux and w0 and modified the Figure 5 and 6 accordingly. I also now provide more robust heat flux uncertainties based on the variability in the observed topographic profile and uncertainties in the stereo-dem. However, and as noted in the text, further uncertainty in the heat flux is also associated with the unknown grain size and strain rate, both of which have a strong effect on the ice shell rheology. A full investigation of these will be the focus of a future study, wherein more topography profiles will also be added.

Considering the regional slope is a great point, that is difficult to address given the available data, but I have added a sentence on this point Line 270:
"*We note that additional geometrical parameters could be considered in the fit to observations, such as with the inclusion of a regional slope, and these would affect the inferred parameters. However, such considerations are difficult to consider given the limited number of available observations and are beyond the scope of this work.*"

Specific items:

Line 121: It isn't true that upward and downward deflection are symmetric in the plastic case, althoguh the yield envelope in fig 1 a is fairly symmetric, fig 1b. shows a difference in the slope of the elastic-plastic curve for positive vs. negative curvature. Is the point here that the elastic-plastic plate has a larger mechanical thickness than the elastic solution's thickness regardless of direction?
True. For simplicity, I have removed this statement.

Line 158: "radially"? This analysis is in Cartesian geometry — what does radially mean? I think this means the curvature varies with distance along the profile.
Indeed, this was meant to imply variations with distance along the profile. Changed to "laterally".

Section 3.2 — it would be good if the authors would recommend a curvature (or amplitude) value below which it is appropriate to use an elastic model and above which the effects of plasticity should be accounted for (in ice). Is it 0.5km? Does it depend strongly on the choice of rheology? This statement should be repeated in the conclusions.

This is a great point. The saturation point very much depends on the rheology and can be calculated by estimating the total bending moment of the yield strength envelope. In general, I observed saturation for curvatures $>10^{-5}$ m^{-1}. Importantly, the curvature also depends on the assumed central deflection (w0) such that solely linking the saturation point to an elastic thickness would not be adequate. This is now discussed line 398-401:
"*The saturation point of the ice shell is a function of its yield strength envelope and can be determined numerically. However, as the ice shell curvature in elastic models depend on both the elastic thickness and central deflection value, it is not possible to provide an elastic thickness threshold above which elastic models are adequate.*"

Line 219: it is precisely this behavior that has allowed sub-glacial lakes in Antarctica to be identified from the surface topography, something that should be referenced here.
Great point, thanks. We added the following statement line 234:
"*On Earth, elevation change have been typically used to subglacial lakes in Antartica (Siegert, 2000; Wilson et al., 2025).*"

and line 409:
"*Thus the presence of local weak zones, such as liquid water pockets, can potentially be detected in elevation data assuming the elastic thickness or ice yield strength is adequately known, as has been done on Earth (Siegert, 2000; Wilson et al., 2025)*"

EDITOR DECISION
round 2

Dear Adrien Broquet,

I would like to thank you for submitting your manuscript "Elastic-plastic flexure and heat flux on icy moons" (#238) to Planetary Research. I have now received the review, which is included at the end of this email. Based on my evaluation of your submission, I believe that your manuscript will be suitable for publication, in principle, following revision.

We ask that you please provide a point-by-point response to all comments and identify what changes you have made to the revised manuscript. In addition to the rebuttal, we ask that you also provide a document that tracks all changes that have been made to your manuscript since its initial submission. Your revisions should be submitted within two months time.

When you have completed your revisions, you can upload the revised manuscript, your response and a tracked changes document from your submission dashboard.

If you have any questions about the recommendations in your review, or if you are unsure how to respond or proceed, please feel free to contact me from your submission dashboard.

Sincerely,

Marie Běhounková
Editor, Planetary Research

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Yes

If you answered no to any of the above questions, please provide supporting information.

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No

Comments for the authors

I would like to thank Dr. Adrien Broquet for the revised manuscript. My primary concerns from the first round of review have been adequately addressed. In particular, the newly added sections provide much greater clarity regarding the proposed methodology, its applicability, and the challenges associated with estimating heat flux.

I have only a few minor comments that I believe would further improve the clarity and readability of the manuscript:

Line 23: I suggest replacing “previously estimated” with “previous estimates” in this context.

Line 32: Should “or Ariel” be changed to “and Ariel”?

Line 381: Could some of the observed deformations also be undergoing viscous relaxation? It may be useful to acknowledge this possibility.

Figure S3 caption: The caption is currently difficult to follow and should be revised for clarity. The description of panel (a) appears to be misleading, as it states, “Same plot, but showing the maximum curvature in color,” whereas the color scale seems to represent heat flux. In addition, panel (c) is not described in the caption, and the “F” is missing from the word “Figure.”

AUTHOR REBUTTAL
round 2

Dear Editor Běhounková and Reviewer 1, I thank you both again for the time taken in assessing my manuscript. You will find below my replies to the comments by Reviewer 1.

Reviewer 1:

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Many thanks!

Line 23: I suggest replacing "previously estimated" with "previous estimates" in this context. Done

Line 32: Should "or Ariel" be changed to "and Ariel"? Changed to "and Ariel"

Line 381: Could some of the observed deformations also be undergoing viscous relaxation? It may be useful to acknowledge this possibility.

I have added the following sentence (line 387): "*We also note that the model does not account for the possibility that observed deformations are undergoing viscoelastic relaxation, which would affect the inferred heat flux*."

Figure S3 caption: The caption is currently difficult to follow and should be revised for clarity. The description of panel (a) appears to be misleading, as it states, "Same plot, but showing the maximum curvature in color," whereas the color scale seems to represent heat flux. In addition, panel (c) is not described in the caption, and the "F" is missing from the word "Figure."

I have clarified the caption as follows:

"Heat flux comparisons at Ariel considering rift flanks investigated in Figure 5. The color provides the heat flux estimated using a maximum curvature criterion for the first (a) and second investigated rift flank (b), which should be compared to the heat flux estimated using a Deborah number in the x-axis. Same plot, but showing the maximum curvature in color (c, d). This figure shows that as curvature increases (b, d), the heat flux estimated using the Deborah number and maximum curvature approach start to depart strongly. The heat flux determined from te maximum curvature is much closer to the elastic-plastic solution shown in Figure 5."