# Peer Review File

**Manuscript Title:** Fine-regolith production on asteroids controlled by rock porosity

## Reviewer Comments & Author Rebuttals

### Reviewer Reports on the Initial Version:

Referees' comments:

Referee #1 (Remarks to the Author):

The idea behind this paper, that porous rocks will be less liable to fragment into dust from micrometeorite impacts, is one of those things that seems painfully obvious — once it has been pointed out — such that the rest of us will be kicking ourselves for not realizing it in the first place.

I've read the article over several times, desperately trying to find something significant to tell the authors… but every time I come up with something, I find that the authors have already anticipated what I was going to say. For example, I was a little worried about the authors using the rule of thumb formula connecting rock porosity with thermal diffusivity, since it is based on empirical data of ordinary chondrites... only to see that the authors already note that fact! While their specific calculations come up with numerical values for how much a rock will be turned to dust feel like a bit of a house made of cards, the fact is that even though the numerical values are guesses at best, the overall qualitative point that the authors are making is spot on.

The importance of the result goes beyond explaining the results of recent spacecraft to also apply in general to future asteroid missions. This paper is both good and important and I endorse its publication in Nature.

— Guy Consolmagno

Referee #2 (Remarks to the Author):

General Comments

Does the manuscript have flaws which should prohibit its publication?
I do not think there is a fundamental flaw in the manuscript, but there is some weaknesses. The nature of the weaknesses is discussed in detail in the following.

Is this work original?
Although thermal properties of Ryugu have been discussed previously by DellaGiustina and Emery et al. (2019) Nature Geoscience and Rozitis et al. (2020) Science Advance, they have not addressed the cause for the low thermal inertia.

Do you feel that the results presented are of immediate interest to many people in your own discipline, or to people from several disciplines?
Yes, I discussed more in detail in the general comments.

A. Summary of the key results
Cambioni et al. conducted very interesting analyses using both thermal infrared and optical data

obtained by NASA's OSIRIS-REx mission. Their results revealed that the thermal inertia of boulders and regolith coverage in surrounding terrains have good correlation. They interpret that this correlation results from the tendancy that high porosity materials lead to less impact ejecta and thermal-fatigue exfoliation. This mechanism can account for unexpectedly low abundance of fine-grained regolith on Ryugu and Bennu surface. This may also apply to other C-type asteroids. Because the physical properties, such as mismatch between low thermal inertia and lack of fine regolith, were among the most important findings of OSIRIS-REx and Hayabsua2 missions, this work is really addressing one of the most fundamental problems of asteroid science now.

B. Originality and significance: if not novel, please include reference
The new data on the correlation between regolith coverage and thermal inertia presented in Fig. 1 is very valuable and useful for understanding the physical properties of such small C-type asteroids, which are widely accepted as the parent body of carbonaceous chondrites. This newly found anticorrelation would be an important clue for understanding the unexpected physical properties of Bennu and Ryugu surfaces.

Because physical properties of asteroid surfaces are extremely important for future missions particularly sample return missions, accurate understanding of surface conditions of asteroids has a great impact to planetary missions of the world. This new physical properties may also influence the selectivity of meteorite collection on Earth. This would further influence the value of future sample return missions for small bodies. Because of such wide range of implications, I believe this manuscript may have the potential to draw strong attention from very broad readers.

Furthermore, I find the data presented in Fig. 1 extremely valuable and process proposed in Fig. 2 is very attractive. Because the anti-correlation between ejecta generation efficiency and target porosity is a widely accepted mechanism in impact physics, the argument that the observed correlation between thermal inertia (proxy for porosity) and regolith coverage may be resulted from this mechanism is very natural. Thus, this study is worth publication. Furthermore, the topic this study is addressing is very fundamental and have far-reaching implications.

Although thermal properties of Ryugu have been discussed previously by DellaGiustina and Emery et al. (2019) Nature Geoscience and Rozitis et al. (2020) Science Advance, they have not addressed the cause for the low thermal inertia.

C. Data & methodology: validity of approach, quality of data, quality of presentation
The data and methodology of analysis are good.

D. Appropriate use of statistics and treatment of uncertainties
I found that the data presented in the manuscript have statistical significance.

E. Conclusions: robustness, validity, reliability
The argument does not appear to be very robust. There is no discussion on corroborating evidence to support this argument. Because OSIRIS-REx carries so many instruments and conducted such a high-resolution imaging observations, I would imagine that there is independent evidence or observation that can test this model. Presentation of corroborating evidence to support Fig. 2 other than Fig. 1 is necessary for publication.

F. Suggested improvements: experiments, data for possible revision
(a) Porosity-driven frustration of impact fracturing 1.
Although anti-correlation known between ejecta generation and target porosity is qualitatively consistent with the observed correlation between thermal inertia (proxy for porosity) and regolith coverage, this study does not present evidence to show that the former is large enough to be the main cause for the latter. Because the degree of the contribution of the proposed mechanism is

not quantitatively discussed, it is difficult to assess the validity of this hypothesis.

(b) Porosity-driven frustration of impact fracturing 2.
Alternatively, the authors could present independent piece of evidence to porosity-driven frustration of impact fracturing. One example I can think of may be the morphological properties of mini-craters on boulders on Bennu. Ballouz et al. (2020) show that mini-craters on boulders on Bennu rarely have spalls, which would produce fractured ejecta. Thus, micrometeorite impacts on Bennu boulders may have produced much smaller amount of ejecta than dense rocks used in many laboratory impact experiments. Such observations could be used to support the claim of this study that low ejecta generation on boulders on Bennu may have contributed to the lack of extensive regolith coverage.

(c) Correlation between thermal inertia and surface abundance of fine regolith
In contrast, the same results by Ballouz et al. (2020) may argue against the model proposed in this study because mini-craters found on Bennu are found only on smooth surface boulders, which are likely have higher thermal inertia according to the analysis of this study. This observation imply that lower porosity boulders on Bennu did not generate much ejecta. Then, the correlation between thermal inertia and regolith coverage is difficult to account for. However, the lack of spalls does not necessarily mean that the impact generated little ejecta; central pit formation can generate ejecta. Thus, I do not believe that this observation kills the model proposed in this study. Nevertheless, this observation does not support the model for the regolith-thermal inertia correlation.

(d) Ejection velocity vs. escape velocity
One important factor not discussed in this study the ejection velocity. Because the surface gravity on Bennu is so low that small launch velocity allows gravels to travel far. Impact ejecta velocity from brittle targets is much higher than that from unconsolidated targets. Thus, it would be difficult to have impact ejecta emplaced in the proximity of the source boulders. Similarly, exfoliation due to thermal fatigue may have high launch velocity. In fact, particle ejection events found on Bennu, which could be due to either meteoritic bombardment or thermal-fatigue-induced exfoliation, have high enough launch velocities to travel more than Bennu diameter (Lauretta and Hergenrother et al. 2019 Science). Maybe I missed something in the manuscript, but I believe a discussion on ejection velocity is needed to account for the correlation between boulder thermal inertia and regolith surface coverage.

G. References: appropriate credit to previous work?
Generally appropriate citations have been given. Minor suggestions are given in the review report.
H. Clarity and context: lucidity of abstract/summary, appropriateness of abstract, introduction and conclusions

Abstract, introduction, and conclusions are written well.

Specific Comments

Line 22-26: "We interpret this finding to mean that accumulation of unconsolidated sub-centimetre particles is frustrated where the rocks are highly porous, which appears to be most of the surface10: these rocks are compacted rather than fragmented by meteoroid impacts, consistent with laboratory experiments11, and thermal cracking proceeds more slowly than in denser rocks".

As the authors cite, reference 11 (Avdellidou et al., 2020) conducted hyper-velocity impact experiments on highly porous CM simulant targets. However, their experimental results show that craters exhibit clear evidence for spallation, which results from fracturing due to impact shock and produce a lot of ejecta. Consequently, experiments by Avdellidou et al. (2020) show that a considerable mass of fragments can be produced. In contrast, detailed observations on Bennu surface by reference 6 (Ballouz et al., 2020) show that "Bennu's boulders show little evidence for spalls". This can be used as evidence to support that a small amount of fractured ejecta is generated by meteoritic impacts on Bennu boulders. Thus, reference 11 should be used evidence against compaction-dominated nature and that reference 6 should be the main supporting evidence.

One complication here is that the mini-craters on Bennu have been found only on smooth boulders (Ballouz et al. 2020), which are estimated to have higher thermal inertia according to the result presented in this study. However, the model proposed in Fig. 2 suggests that these smooth boulders are more prone to produce fractures upon impacts. This model prediction appear to be rather inconsistent with the lack of spalls in Bennu's mini-craters. Here, it is noted that mini-craters on hummocky boulders, which have lower thermal inertia, have not been found because search for surface cavities on hummocky surfaces is extremely difficult. Thus, it is still possible that micrometeorite bombardments on hummocky boulders may end up possessing mini-craters that would have produced even less ejecta than smooth boulders. However, there is no observational evidence for this; what we know now based on the observations by Ballouz et al. (2020) appear to be rather inconsistent with the model.

A related comment is given below on comparison of the morphologies of boulder craters on Bennu and Ryugu.

Line 29-30: "The higher porosity of carbonaceous asteroid materials may have aided in the formation of breccias that dominate the carbonaceous chondrite meteorites13". The phrase "the formation of breccias" might be a bit confusing here, because this phrase could refer to all the stages of breccia formation process, from fracturing, mixing, compaction, and cementation. I believe that the authors are intended to say compaction and/or cementation here to produce low-porosity breccias found in Earth collection as stated in the conclusion. It might be good to specify that what the mechanism proposed in this study is to convert high-porosity materials into breccias via compaction. It is also worth mentioning that many breccias have been found on both Bennu and Ryugu (Walsh et al., 2019, Sugita et al. 2019). If this is too much to add in the abstract, the last sentence (line 146-148) may be a good place to mention actual breccias on Bennu and Ryugu.

Line 87: "Laboratory experiments of hypervelocity impacts on meteorites22 and meteorite simulants11,23 demonstrate that particle ejecta production and target break-up are different for collisions onto targets with different porosities". I wonder if this sentence would be easier to understand if it is rephrased using the words "higher" and "lower". One example would be: "Laboratory experiments of hypervelocity impacts on meteorites and meteorite simulants demonstrate that more particle ejecta is produced from lower porosity targets and that more compaction occur on higher porosity targets".
I wonder in what sense the reference 23 (Avdellidou et al. 2020) is demonstrating the argument here. I mean, Avdellidou et al. 2020 is a good paper conducting interesting carbonaceous chondrite simulant with high porosity. However, I do not think that they measured ejecta production or compaction effects. Because they are observing the crater morphologies on highly porous targets, target break up process was investigated. However, I did not find their data are necessarily support the arguments in this study. This, this citation does not seem to be appropriate here. I find that Avdellidou et al. (2020) is good to cite in this study, but it should be cited in an appropriate context.

line 107: "Evidence from other asteroids" Regarding comparison with other asteroids, morphology of impact craters on boulders on Ryugu may be worth discussing. Although morphologic analysis of craters on Ryugu boulders is not as advanced as that for Bennu, there have been some reports. One is a large cavity, whose morphology is consistent with spall, central pit, and radial fracture, on the largest boulder (Otohime) on Ryugu (Fig. 4F, Sugita et al. 2019). The other is Takai et al. (2021) LPSC #2548. They found mini-craters on smooth boulders on Ryugu, and they seem to possess spalls and central pits. The presence of spalls-like structures on craters on smooth boulders (types 2 and 4 boulders) on Ryugu might be more consistent with the argument by Cambioni et al.; more fracturing on smooth boulders.

Thus, fracturing and compaction due to meteoritic impacts on C-type asteroids may be more complicated than the authors are claiming. Such mismatch between their model prediction and observations on Bennu and Ryugu should be discussed as either supporting evidence or caveat in the manuscript.

Line 110: "Measurements by JAXA's Hayabusa2 mission18,25 indicate that the largest boulders on Ryugu have thermal inertia ΓR ~ 300 MKS, while only a few boulders with ΓR ~ 600–1,000 MKS18". There is a publication for more precise measurements by Shimaki et al. (2020) Icarus. 348, 113835. This study also provides the roughness distribution on Ryugu (Fig. 9).

Line 123: Itokawa has extensive areas covered in centimetre-sized gravels2, while Bennu's and Ryugu's surfaces do not8,9. This statement is also a bit oversimplified. Although the contrasting difference has been clearly found between Itokawa and the other two in the surface areas of smooth terrains with cm-sized gravels, there is a caveat about the difference. The presence and absence of smooth terrains with cm-sized gravels is that the difference may be greatly controlled by size sorting. Then the total abundance of cm-size gravels on the entire globes may not be much different.
It would be good to add Miyamoto et al. (2007) Science, 316,1011-1014 here because reference 2 covers only down to several meters of resolution. However, reference 2 is the paper that reports the presence of smooth terrains on Itokawa.

**Author Rebuttals to Initial Comments:**

**Answer to referees' report for Cambioni et al.'s Nature manuscript 2021-03-04081**

**Title: "Fine-regolith production on asteroids controlled by rock porosity"**

We thank both reviewers and the editor of Nature for providing us with valuable comments and suggestions to improve our manuscript. We believe that we implemented all of them. The main changes to our manuscript are (i) clarifying corroborating evidence for the correlation between thermal inertia/porosity of rocks and the abundance of fine regolith; (ii) providing further evidence about the different porosities of rocks on Bennu from independent measurements; (iii) making the explanation about rock fragmentation by meteorite impacts more coherent by discussing the production of spalls and the speed of ejecta; (iv) fixing and adding references to properly support our statements. In particular, points (i) and (ii) provide further evidence for the robustness of our findings.

Please find in the following our detailed answers point by point. **To ease the reading of this reply letter, the reviewers' comments are in *italic*, while our responses are indented in plain text.**

*Referee #1 (Remarks to the Author):*

*The idea behind this paper, that porous rocks will be less liable to fragment into dust from micrometeorite impacts, is one of those things that seems painfully obvious — once it has been pointed out — such that the rest of us will be kicking ourselves for not realizing it in the first place.*

*I've read the article over several times, desperately trying to find something significant to tell the authors… but every time I come up with something, I find that the authors have already anticipated what I was going to say. For example, I was a little worried about the authors using the rule of thumb formula connecting rock porosity with thermal diffusivity, since it is based on empirical data of ordinary chondrites... only to see that the authors already note that fact! While their specific calculations come up with numerical values for how much a rock will be turned to dust feel like a bit of a house made of cards, the fact is that even though the numerical values are guesses at best, the overall qualitative point that the authors are making is spot on.*

*The importance of the result goes beyond explaining the results of recent spacecraft to also apply in general to future asteroid missions. This paper is both good and important and I endorse its publication in Nature.*

*— Guy Consolmagno*

We thank Dr. Consolmagno for the comments and the endorsement.

*Referee #2 (Remarks to the Author):*

*General Comments*

*Does the manuscript have flaws which should prohibit its publication?*

*I do not think there is a fundamental flaw in the manuscript, but there is some weaknesses. The nature of the weaknesses is discussed in detail in the following.*

*Is this work original?*

*Although thermal properties of Ryugu have been discussed previously by DellaGiustina and Emery et al. (2019) Nature Geoscience and Rozitis et al. (2020) Science Advance, they have not addressed the cause for the low thermal inertia.*

> We would like to add that our paper is the first to address the reason for the scarcity of fine regolith on the surface of Bennu and Ryugu and to directly measure the thermal inertia and abundance of rocks and fine regolith on Bennu within the OTES instrument's footprint. Previous studies of Bennu's surface did not distinguish between the thermal inertia values of boulders/rocks and finer material, nor they directly measured their thermal properties, except for the one case where the OTES footprint is almost entirely filled by the largest boulder.

*Do you feel that the results presented are of immediate interest to many people in your own discipline, or to people from several disciplines?*

*Yes, I discussed more in detail in the general comments.*

*A. Summary of the key results*

*Cambioni et al. conducted very interesting analyses using both thermal infrared and optical data obtained by NASA's OSIRIS-REx mission. Their results revealed that the thermal inertia of boulders and regolith coverage in surrounding terrains have good correlation. They interpret that this correlation results from the tendancy that high porosity materials lead to less impact ejecta and thermal-fatigue exfoliation. This mechanism can account for unexpectedly low abundance of fine-grained regolith on Ryugu and Bennu surface. This may also apply to other C-type asteroids. Because the physical properties, such as mismatch between low thermal inertia and lack of fine regolith, were among the most important findings of OSIRIS-REx and Hayabsua2 missions, this work is really addressing one of the most fundamental problems of asteroid science now.*

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

*The new data on the correlation between regolith coverage and thermal inertia presented in Fig. 1 is very valuable and useful for understanding the physical properties of such small C-type asteroids, which are widely accepted as the parent body of carbonaceous chondrites. This newly found anticorrelation would be an important clue for understanding the unexpected physical properties of Bennu and Ryugu surfaces.*

*Because physical properties of asteroid surfaces are extremely important for future missions particularly sample return missions, accurate understanding of surface conditions of asteroids has a great impact to planetary missions of the world. This new physical properties may also influence the selectivity of meteorite collection on Earth. This would further influence the value of future sample return missions for small bodies. Because of such wide range of implications, I believe this manuscript may have the potential to draw strong attention from very broad readers.*

*Furthermore, I find the data presented in Fig. 1 extremely valuable and process proposed in Fig. 2 is very attractive. Because the anti-correlation between ejecta generation efficiency and target porosity is a widely accepted mechanism in impact physics, the argument that the observed correlation between thermal inertia (proxy for porosity) and regolith coverage may be resulted from this mechanism is very natural. Thus, this study is worth publication. Furthermore, the topic this study is addressing is very fundamental and have far-reaching implications.*

*Although thermal properties of Ryugu have been discussed previously by DellaGiustina and Emery et al. (2019) Nature Geoscience and Rozitis et al. (2020) Science Advance, they have not addressed the cause for the low thermal inertia.*

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

*The data and methodology of analysis are good.*

*D. Appropriate use of statistics and treatment of uncertainties*

*I found that the data presented in the manuscript have statistical significance.*

We thank the reviewer for the insightful comments and inputs. We provide answers to specific comments below.

*E. Conclusions: robustness, validity, reliability*

*The argument does not appear to be very robust. There is no discussion on corroborating evidence to support this argument. Because OSIRIS-REx carries so many instruments and conducted such a high-resolution imaging observations, I would imagine that there is independent evidence or observation that can test this model. Presentation of corroborating evidence to support Fig. 2 other than Fig. 1 is necessary for publication.*

We agree that the presentation of corroborating evidence to support Fig. 2 of the submitted version (now Fig. 3) other than Fig. 1 is necessary. Indeed, we provide supporting evidence obtained from analysis of instruments (PolyCam and OLA) that are independent from the OSIRIS-REx Thermal Emission Spectrometer (OTES). In particular:

- Already in the submitted version of our manuscript, from PolyCam optical images, we measured the abundance of unresolved material (as the sum of areas that visually do not look like rocks) at two of the potential sampling sites and in two other areas (this was also noted by the reviewer's comment about optical data in the point "A. Summary of the key results"). We find that the abundance of unresolved material is consistent with the abundance of fine regolith, the latter calculated from our machine learning thermophysical analysis of OTES data. We present this at lines 67–71 of the main text and in Extended Data Figure 2 and 3; we describe the procedures adopted in the Method section "Tests of the robustness of the results".

- To take into further account this comment by the reviewer, as well as comments (F.b) and (F.c) below, we added at lines 98–103 of the revised manuscript a discussion on the observation that the depth-to-diameter (d/D) ratio of the mini-craters observed by Ballouz et al. (2020) is different for different types of boulders. Ballouz et al. mapped craters on both smooth-textured and hummocky-textured boulders in the global image mosaic at 5 cm/pixel. OSIRIS-REx Laser Altimeter (OLA) measurements of the morphology of these craters yielded d/D = 0.33 +- 0.08, with the hummocky boulders having larger d/D values than smooth boulders. The latter was confirmed by mapping craters on smooth-textured boulders only using limited-coverage, higher-resolution images (this second mapping campaign yielded d/D ~ 0.25 for the smooth boulders). We interpret this as corroborating evidence that Bennu hosts rocks of different porosities because d/D typically increases with the porosity of the target (e.g., Michikami et al. 2007 P&SS 55, 70–88 (2007), Flynn et al. 2015 P&SS 107, 64–76). Since Rozitis et al. 2020 report that large hummocky boulders could have lower thermal inertia (and thus higher porosity) than the smaller, smoother boulders, the craters' d/D value varies consistently with the thermal inertia of boulders.

*F. Suggested improvements: experiments, data for possible revision*

*(a) Porosity-driven frustration of impact fracturing 1.*

*Although anti-correlation known between ejecta generation and target porosity is qualitatively consistent with the observed correlation between thermal inertia (proxy for porosity) and regolith coverage, this study does not present evidence to show that the former is large enough to be the main cause for the latter. Because the degree of the contribution of the proposed mechanism is not quantitatively discussed, it is difficult to assess the validity of this hypothesis.*

This is an important point, which led us to perform additional work leading to re-wording the paragraph of the main text where we describe how fine regolith production from meteoroid impacts depends on porosity (lines 98-111). Our investigation of the literature shows that there is no doubt that ejecta production from meteoroid impacts decreases with increasing porosity of the impacted materials, as it has been also pointed out by the reviewer in their comment B. However, a precise functional form for our case is not available yet. We detail this in the following.

First of all, we point out in the revised version, and further down in this answer to reviewers' comments, that (i) according to current understanding of impact physics, little ejecta from vertical impacts on Bennu-like rocks would have velocity below Bennu's gravitational escape speed. At the same time, impacts occurring in nature happen at some angle on boulders leading to ejecta being retained by Bennu; (ii) it is likely that impacts of meteoroids exceeding the energy for boulder breakup will not only produce ejecta, but also fragment in situ the boulder without launching the fragments; (iii) the correlation of Fig. 1 is not only due to collisional breakup but also to thermal fragmentation.

Concerning ejecta production, there are studies based on laboratory experiments to quantify the amount of ejecta generation as a function of target porosity (ϕ). One of these is Housen et al. (2018), Impacts into porous targets. Icarus 300, p. 72-96. In the case of cratering, using an empirical formula, Housen et al. quantify how the ratio between the mass of ejecta (Me) and crater mass (Mc), i.e. Me/Mc, decreases with decreasing material crushing strength (Yc). Housen et al. show the relationship between Yc and ϕ too. So, in principle, we could express Me/Mc vs. ϕ. If we do this, we find that the mass of ejecta decreases by some factors as ϕ increases from a few % to ~ 60% (the latter is approximately the range of ϕ from our Fig. 1). So, regardless of exact numbers, we believe that this could be a solid quantitative argument in agreement with our Figure 1.

However, the exact amount of decrease of Me/Mc vs. Yc (and vs. ϕ) depends on the lithostatic overburden (ρgh) in the case of the unconsolidated soil of Housen et al. Our problem of cratering on rocks should be better represented by studying the Me/Mc vs. the ratio between shear and crushing strength (i.e., vs. Ys/Yc, instead of ρgh/Yc). This is because, in a rock, it is shearing that competes against compaction, but this quantification is not available in literature from our best knowledge. In this respect, we encourage more laboratory experiments to be performed in simulants as those of Avdellidou et al. 2020. Additionally, we caution that the Housen et al.'s empirical formula is validated for values of surface gravity larger than that of Bennu, Ryugu and Itokawa, and tested for granular unconsolidated materials. For these reasons, we cite Housen et al. (2018) in the paper but we prefer to not use it directly to form a quantitative description of regolith production on Bennu.

Furthermore, there is another important source of fine regolith in addition to impact ejecta: in situ breakdown of rocks via collisional and thermal fragmentation. We discuss this on lines 158–161 of the revised manuscript and we provide images of boulders that experienced in-situ fragmentation in the new Extended Data Figure 9. We also edited our Fig. 3 (previously Fig. 2) to make this point clearer. As far as thermal cracking, Delbo et al. 2014 showed that it could play even a more important role than the meteoroid impacts in the production of small particles from the comminution of rocks (and here we show that thermal cracking also depends on rock porosity).

Given the above and the effects of both mechanisms, impacts and thermal cracking, we believe that the “quantification” problem of how much fine regolith is produced as function of rock porosity should be addressed in the opposite direction: the results of our Fig. 1 can be used as a constraint to develop a quantitative relationship between rock porosity and fine-regolith production. We believe that the latter should be addressed in forthcoming papers. These works will need to extend the studies of Housen et al. (2018) to smaller surface gravities, consolidated targets (i.e. developing relationships between Me and Ys/Yc), and estimating the regolith sizes and amount produced by thermal cracking, before comparing them to our OSIRIS-REx results of Fig. 1.

*(b) Porosity-driven frustration of impact fracturing 2.*

*Alternatively, the authors could present independent piece of evidence to porosity-driven frustration of impact fracturing. One example I can think of may be the morphological properties of mini-craters on boulders on Bennu. Ballouz et al. (2020) show that mini-craters on boulders on Bennu rarely have spalls, which would produce fractured ejecta. Thus, micrometeorite impacts on Bennu boulders may have produced much smaller amount of ejecta than dense rocks used in many laboratory impact experiments. Such observations could be used to support the claim of this study that low ejecta generation on boulders on Bennu may have contributed to the lack of extensive regolith coverage.*

This is a great suggestion by the reviewer. We implement it at lines 107-110 of the revised manuscript as we detail here further below.

*(c) Correlation between thermal inertia and surface abundance of fine regolith*

*In contrast, the same results by Ballouz et al. (2020) may argue against the model proposed in this study because mini-craters found on Bennu are found only on smooth surface boulders, which are likely have higher thermal inertia according to the analysis of this study. This observation imply that lower porosity boulders on Bennu did not generate much ejecta. Then, the correlation between thermal inertia and regolith coverage is difficult to account for. However, the lack of spalls does not necessarily* *mean that the impact generated little ejecta; central pit formation can generate ejecta. Thus, I do not believe that this observation kills the model proposed in this study. Nevertheless, this observation does not support the model for the regolith-thermal inertia correlation.*

The reviewer is partially correct when they state that observations of mini-craters were limited to smooth boulders, as this is true for the higher-resolution image set (1–3 cm/pixel). However, some craters were observed on boulders with relatively rough and hummocky texture in the lower-resolution image set (~ 5 cm/pixel). Hence the results by Ballouz et al. (2020) do not argue against the model proposed in this study because mini-craters on Bennu are found on all types of boulders.

In particular, the images of boulders on craters presented in Ballouz et al. (2020) came from two main sources:

1. Limited coverage images of the surface at ~1–3 cm/pixel scale taken during the Orbital phase of the mission, where illumination conditions allowed for the observations of small and shallow craters (3 - 50 cm) on the surfaces of flat boulders (~ 2 m in diameter).

2. Global coverage images ~5 cm/pixel, where illumination conditions limited the size of the craters that could be observed.

In the case of observations from source (2), crater observations were confirmed and their diameters and depth measured by using 3D point cloud data returned from the OSIRIS-REx Laser Altimeter (OLA). These craters ranged in size from ~15 cm to 5 m. We show an example of a crater on a hummocky boulder in the following image, also shown in Ballouz et al. 2020 (Fig. 1c).


We note that this boulder has rough undulations on its surface and also appears to be brecciated (see the large brighter clast in top right corner, and smaller ones scattered throughout). This is consistent with our assertions in the final paragraph of the manuscript (lines 178–180).

Following the suggested improvements by the reviewer, we comment on the above on lines 98–101.

*(d) Ejection velocity vs. escape velocity*

*One important factor not discussed in this study the ejection velocity. Because the surface gravity on Bennu is so low that small launch velocity allows gravels to travel far. Impact ejecta velocity from brittle targets is much higher than that from unconsolidated targets. Thus, it would be difficult to have impact ejecta emplaced in the proximity of the source boulders. Similarly, exfoliation due to thermal fatigue may have high launch velocity. In fact, particle ejection events found on Bennu, which could be due to either meteoritic bombardment or thermal-fatigue-induced exfoliation, have high enough launch velocities to travel more than Bennu diameter (Lauretta and Hergenrother et al. 2019 Science). Maybe I missed something in the manuscript, but I believe a discussion on ejection velocity is needed to account for the correlation between boulder thermal inertia and regolith surface coverage.*

We agree with the reviewer that a more in-depth discussion on ejection velocity is needed; we added it on lines 151–164.

As the reviewer pointed out, according to the current understanding of ejecta production, average ejecta velocity decreases with increasing porosity of the targets (e.g., see Michikami et al, Planetary and Space Science 55, 70–88 (2007)). From these experiments, we deduce that accumulation of fine regolith should be favoured on Bennu and Ryugu, whose rocks are more porous compared to Itokawa's, but extensive areas covered in fine regolith were only observed on the latter, suggesting that more fine regolith is produced on Itokawa to compensate for the higher losses. We also point out that if fine regolith would strongly distribute isotropically on Bennu from each local source, then there would be no reason to observe the inverse correlation between the rock thermal inertia and the local surface abundance of fine regolith of Fig 1, which is statistically robust.

Our current understanding of impact ejecta on rocks is that little mass should be retained in vertical impacts on rocks at the low gravities of small asteroids. However, there are a few important points to consider: (1) The most common impact angle is more likely to be 45 degrees, which may emplace some ejecta on the nearby surface. (2) Even in the case that no ejecta is retained, the observation of rocks being broken in tightly-clustered pieces on Bennu suggest that regolith can be produced with minimal displacement by in-situ fragmentation of large rocks exposed on the surface (the process is somewhat analogous to what has been observed on the Moon, e.g., Ruesch et al. 2020 Icarus 336, 113431). We comment on this on lines 158–161 and we provide images in the new Extended Data Figure 9; and (3) In the laboratory, it is very difficult to get an accurate measurement of ejecta that would remain near the rock after impact, besides maybe large-ish fragments and dust that stays near the impact point. Experiments would need to be able to measure ejecta being launched at ~ 10 cm/s. Even if an experiment could measure such low-speeds, a fragment launched at 45 deg with v_total = 10 cm/s, would reach a maximum vertical height of < 1cm before falling back. We hope that our paper will inspire more laboratory work aimed to better constrain low-speed ejecta production at the low escape velocities typical of small asteroids.

Additionally, we now discuss the ejection of regolith particles during rock fragmentation by thermal exfoliation on lines 155–156 and cite on line 152 Molaro et al. 2020 (JGR, e2019JE006325), who also reported on the widespread presence of thermal fragmentation signatures on Bennu. We point out that the exfoliation described in the model of that paper is only one aspect of thermal fragmentation. Furthermore, the Molaro et al.'s model predicts rather high thermal stresses (some MPa from their Figure 2) compared to our understanding of boulder strengths of some fractions of MPa on carbonaceous asteroids (Grott et al. 2019, Ballouz et al. 2020). In this respect, it seems that the Molaro et al.'s mechanism is hard to reconcile with fatigue crack growth, which by definition happens at stresses lower than the strength of the material. Thermal fatigue regolith formation happens at subcritical stresses (the stress intensity factor < material strength) and therefore is likely more gentle; Delbo et al. 2014 indeed observed small fragments of the meteoritic material that fell off in the sample holder.

*G. References: appropriate credit to previous work?*

*Generally appropriate citations have been given. Minor suggestions are given in the review report.*

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

*Abstract, introduction, and conclusions are written well.*

*Specific Comments*

*Line 22-26: "We interpret this finding to mean that accumulation of unconsolidated sub-centimetre particles is frustrated where the rocks are highly porous, which appears to be most of the surface10: these rocks are compacted rather than fragmented by meteoroid impacts, consistent with laboratory experiments11, and thermal cracking proceeds more slowly than in denser rocks".*

*As the authors cite, reference 11 (Avdellidou et al., 2020) conducted hyper-velocity impact experiments on highly porous CM simulant targets. However, their experimental results show that craters exhibit clear evidence for spallation, which results from fracturing due to impact shock and produce a lot of ejecta. Consequently, experiments by Avdellidou et al. (2020) show that a considerable mass of fragments can be produced. In contrast, detailed observations on Bennu surface by reference 6 (Ballouz et al., 2020) show that "Bennu's boulders show little evidence for spalls". This can be used as evidence to support that a small amount of fractured ejecta is generated by meteoritic impacts on Bennu boulders. Thus, reference 11 should be used evidence against compaction-dominated nature and that reference 6 should be the main supporting evidence.*

> We agree with the reviewer that Avdellidou et al. is not a good citation here. We think that there is a misunderstanding because we do not cite Avdellidou et al. in the summary paragraph. Citation 11 was referring to Flynn et al. 2015. Namely, the reference is:
>
> Flynn, G. J. et al. Hypervelocity cratering and disruption of porous pumice targets: Implications for crater production, catastrophic disruption, and momentum transfer on porous asteroids. Planetary and Space Science 107, 64–76 (2015).

On the same line 29, we also added a citation to Housen et al. (2018, Impacts into porous targets. *Icarus* 300, p. 72-96) to comment on impact compression. We cite Avdellidou later on in the paper at line 108 where we talk about spalling.

*One complication here is that the mini-craters on Bennu have been found only on smooth boulders (Ballouz et al. 2020), which are estimated to have higher thermal inertia according to the result presented in this study.*

We would like to comment about the misconception that Ballouz et al. 2020 found mini-craters only on smooth boulders, which is not the case. Ballouz et al. found mini-craters also on hummocky boulders (see our previous comments). The craters are just more difficult to observe and measure on the hummocky surfaces, not absent. Ballouz et al. use "rough" to refer to the boulder type that we call "hummocky" here. Furthermore, in our paper we do not claim that smooth boulders have higher thermal inertia than hummocky ones, though this statement is not in contrast with our work. That statement comes from the analysis of OTES data by Rozitis et al. 2020.

*However, the model proposed in Fig. 2 suggests that these smooth boulders are more prone to produce fractures upon impacts. This model prediction appear to be rather inconsistent with the lack of spalls in Bennu's mini-craters. Here, it is noted that mini-craters on hummocky boulders, which have lower thermal inertia, have not been found because search for surface cavities on hummocky surfaces is extremely difficult.*

Please see our previous answer. We believe that there is no contradiction between this study and that of Ballouz et al. 2020.

Actually, following the valuable suggestion of the reviewer (point F.b), we now use the general lack of spalls of Ballouz et al. mini-craters as an additional evidence that ejecta production from impact of meteoroids is frustrated by Bennu's boulder's high porosity (lines 107–110), compared to laboratory experiments that often show spalls in denser materials. As the reviewer pointed out before, one example of the latter is the work by Avdellidou et al. 2020, who used rocks with the similar mineralogy to the (few) boulders of Bennu that have porosities ~25%. This is a relatively low value with respect to the bulk of Bennu's rocks (Figure 2), which explains why Avdellidou et al. could see spalls more often than what Ballouz et al 2020 observed. Moreover, following the reviewer's comment E (conclusions: robustness, validity, reliability), we now also describe the variability of the depth to diameter ratio of mini craters of Ballouz et al. as evidence of diversity of boulder porosity on Bennu (lines 99–103).

*Thus, it is still possible that micrometeorite bombardments on hummocky boulders may end up possessing mini-craters that would have produced even less ejecta than smooth boulders. However, there is no observational evidence for this; what we know now based on the observations by Ballouz et al. (2020) appear to be rather inconsistent with the model.*

We agree with the first sentence of the above comment from the reviewer. Rocks on Bennu, regardless of their type, should suffer similar bombardment and thus production of mini-craters. Furthermore, hummocky boulders on Bennu tend to be larger than the smooth boulders (DellaGiustina, Emery et al. 2019). We also agree with the reviewer that the identification of mini-craters on hummocky rocks is more challenging than on smooth rocks, so there is an obvious bias against their visual detection on this geological unit compared to the smooth rocks.

However, we respectfully disagree with the second comment, because there is observational evidence for the presence of micrometeorite bombardments on hummocky boulders (as we commented before).

*A related comment is given below on comparison of the morphologies of boulder craters on Bennu and Ryugu.*

*Line 29-30: "The higher porosity of carbonaceous asteroid materials may have aided in the formation of breccias that dominate the carbonaceous chondrite meteorites13". The phrase "the formation of breccias" might be a bit confusing here, because this phrase could refer to all the stages of breccia formation process, from fracturing, mixing, compaction, and cementation. I believe that the authors are intended to say compaction and/or cementation here to produce low-porosity breccias found in Earth collection as stated in the conclusion. It might be good to specify that what the mechanism proposed in this study is to convert high-porosity materials into breccias via compaction. It is also worth mentioning that many breccias have been found on both Bennu and Ryugu (Walsh et al., 2019, Sugita et al. 2019). If this is too much to add in the abstract, the last sentence (line 146-148) may be a good place to mention actual breccias on Bennu and Ryugu.*

These are great comments. We indeed meant to refer to the process that the referee mentioned. We implemented the referee's suggestions in the summary paragraph (line 34–36) and in the conclusions section of the main text (line 180).

*Line 87: "Laboratory experiments of hypervelocity impacts on meteorites22 and meteorite simulants11,23 demonstrate that particle ejecta production and target break-up are different for* *collisions onto targets with different porosities". I wonder if this sentence would be easier to understand if it is rephrased using the words "higher" and "lower". One example would be: "Laboratory experiments of hypervelocity impacts on meteorites and meteorite simulants demonstrate that more particle ejecta is produced from lower porosity targets and that more compaction occur on higher porosity targets".*

*I wonder in what sense the reference 23 (Avdellidou et al. 2020) is demonstrating the argument here. I mean, Avdellidou et al. 2020 is a good paper conducting interesting carbonaceous chondrite simulant with high porosity. However, I do not think that they measured ejecta production or compaction effects. Because they are observing the crater morphologies on highly porous targets, target break up process was investigated. However, I did not find their data are necessarily support the arguments in this study. This, this citation does not seem to be appropriate here. I find that Avdellidou et al. (2020) is good to cite in this study, but it should be cited in an appropriate context.*

We edited lines 104–106 as suggested by the reviewer. We also agree to remove the citation to Avdellidou et al. 2020. We intended to not cite that paper here too; it was an omission during the iterations with the co-authors. We still cite Avdellidou et al. on line 108 when talking about spalls.

*line 107: "Evidence from other asteroids" Regarding comparison with other asteroids, morphology of impact craters on boulders on Ryugu may be worth discussing. Although morphologic analysis of craters on Ryugu boulders is not as advanced as that for Bennu, there have been some reports. One is a large cavity, whose morphology is consistent with spall, central pit, and radial fracture, on the largest boulder (Otohime) on Ryugu (Fig. 4F, Sugita et al. 2019). The other is Takai et al. (2021) LPSC #2548. They found mini-craters on smooth boulders on Ryugu, and they seem to possess spalls and central pits. The presence of spalls-like structures on craters on smooth boulders (types 2 and 4 boulders) on Ryugu might be more consistent with the argument by Cambioni et al.; more fracturing on smooth boulders.*

We added a reference to Sugita et al. 2019, Science, in the summary paragraph (line 23) to indicate that signatures of meteoroid bombardments are also observed on Ryugu and its largest boulder, Otohime. Takai et al. presents very interesting statistics of the small craters observed on boulders' surfaces on Ryugu. However, we could not comment on the comparison between Bennu's and Ryugu's mini-craters because not enough information is provided in the Takai et al. abstract about the morphology of the mini-craters on Ryugu.

*Thus, fracturing and compaction due to meteoritic impacts on C-type asteroids may be more complicated than the authors are claiming. Such mismatch between their model prediction and observations on Bennu and Ryugu should be discussed as either supporting evidence or caveat in the manuscript.*

The reviewer is correct stating that fracturing and compaction mechanisms are complicated. Indeed, during the revision, in order to satisfy other specific comments, we extended our discussion to other aspects. Now we discuss: (1) the misunderstanding about spalling; (2) the d/D ratio of the mini-crater; (3) the in-situ fragmentation of rocks. Finally, we further elaborate on ejecta production and speeds.

*Line 110: "Measurements by JAXA's Hayabusa2 mission18,25 indicate that the largest boulders on Ryugu have thermal inertia ΓR ~ 300 MKS, while only a few boulders with ΓR ~ 600–1,000 MKS18". There is a publication for more precise measurements by Shimaki et al. (2020) Icarus. 348, 113835. This study also provides the roughness distribution on Ryugu (Fig. 9).*

We thank the reviewer for this comment. We rephrase the text on lines 132–136 to include the citation to Shimaki et al. (2020) Icarus. 348, 113835 and update the description of the thermal inertia measurements for Ryugu.

*Line 123: Itokawa has extensive areas covered in centimetre-sized gravels2, while Bennu's and Ryugu's surfaces do not8,9. This statement is also a bit oversimplified. Although the contrasting difference has been clearly found between Itokawa and the other two in the surface areas of smooth terrains with cm-sized gravels, there is a caveat about the difference. The presence and absence of smooth terrains with cm-sized gravels is that the difference may be greatly controlled by size sorting. Then the total abundance of cm-size gravels on the entire globes may not be much different. It would be good to add Miyamoto et al. (2007) Science, 316,1011-1014 here because reference 2 covers only down to several meters of resolution. However, reference 2 is the paper that reports the presence of smooth terrains on Itokawa.*

As the reviewer correctly points out, global-size sorting on Itokawa could control the presence of smooth terrains on its surface. However, as Miyamoto et al. (2007) point out, this size-sorting proceeds through mechanisms that induce surface mass motion. As Itokawa is approximately the same size as Bennu, these mechanisms may operate with similar efficiency. Furthermore, mass motion has been observed and reported for Bennu (Jawin et al. 2020) as well as Ryugu (Sugita et al. 2019). The lack of smooth terrains on Bennu and Ryugu, combined with strong evidence for surface mass-motion, would suggest that these asteroids may have a lower global abundances of fine regolith compared to Itokawa. Our study proposes a mechanism that would lead to this differential in cm-sized particle abundance between the asteroid types.

We comment on the above on lines 145–149. We also added a citation to Miyamoto et al. (2007) Science, 316,1011-1014 in the paper.

**Reviewer Reports on the First Revision:**

Referees' comments:

Referee #1 (Remarks to the Author):

Almost ready to go, I only have three minor comments:

Line 102 I think should read "consistent with" rather than "consistently with"

Reference 37: the wrong journal is cited, it is Chemie der Erde not Geochemistry (the rest of the reference is correct)

Caption to Figure 2 seems to be missing a word, should read "Porosity of most of Bennu's..."

-- Guy Consolmagno

Referee #2 (Remarks to the Author):

General Comment

I have read the revised manuscript and rebuttal letter and found that the authors did a great job revising the manuscript. I also like to indicate that some of my criticism was based on my misunderstanding about the mini-crater observations by Ballouz and that the authors explained very well. Now I really endorse the contents of this manuscript and support publication in Nature. I have only one minor recommendation for further revision of the manuscript. That is the effect of impact angle discussed in "(d) Ejection velocity vs. escape velocity". Nevertheless, this disagreement does not affect the main conclusions of this study. This should not stop the publication process.

>> The argument does not appear to be very robust. There is no discussion on
>> corroborating evidence to support this argument. Because OSIRIS-REx carries so
>> many instruments and conducted such a high-resolution imaging observations, I
>> would imagine that there is independent evidence or observation that can test this
>> model. Presentation of corroborating evidence to support Fig. 2 other than Fig. 1 is
>> necessary for publication.
> We agree that the presentation of corroborating evidence to support Fig. 2 of the
> submitted version (now Fig. 3) other than Fig. 1 is necessary. Indeed, we provide
> supporting evidence obtained from analysis of instruments (PolyCam and OLA) that
> are independent from the OSIRIS-REx Thermal Emission Spectrometer (OTES). In
> particular:
I really appreciate the added evidence to support difference in d/D ratios of the mini-craters and discussion in the revised manuscript. I find this morphometric evidence to support the difference in response to meteoritic impacts between two types of boulders very convincing, mostly because the nature of this evidence is really independent of the data presented in Figure 1.

The following may be beside the point, but I agree with the authors that Extended Data Figures 2 and 3 also provide evidence to support Figure 1; my statement that no corroborating evidence was an overstatement. However, although Extended Data Figures were supportive, I did not find the data presented in these figures were very strong. Nevertheless, the above addition of the/D ratio data takes care of my concern. Now you have three pieces of evidence.

(a) Porosity-driven frustration of impact fracturing 1.
I appreciate the authors' revision on the argument on the ejecta generation and attempt for quantitative assessment. I had an impression that the authors are considering that classic impact ejecta launched upward from the crater cavity created from meteoroidal collisions as the main contributor to the correlation between regolith abundance and thermal inertia. After the revision, however, it is clear that authors are also considering in-situ fragmentation due to meteoroidal impacts. The observation that Bennu's mini-craters rarely have spalls also helps this argument.

After the authors' careful revision on impact processes in this paragraph, I find quantitative assessment on impact ejecta mass as a function of porosity unnecessary for this manuscript. The reason why I thought such quantitative assessment is necessary is that I thought that the authors were considering classic impact ejecta and this component should not account for a large portion of surface regolith on Bennu. I think this concern has been resolved.

(b) Porosity-driven frustration of impact fracturing 2.
Good to see the suggestion has been adopted.

(c) Correlation between thermal inertia and surface abundance of fine regolith
Thank you for pointing out my misunderstanding. I also found the revised text will prevent misunderstanding similar to mine.

(d) Ejection velocity vs. escape velocity
I found that the authors did a good job resolving the concerns I had about impact ejecta velocity by revising this paragraph. I also agree with the argument about the thermal fatigue regolith formation based on the observation that low boulder strengths estimates on Bennu and Ryugu.

In contrast, I do not think that ejecta launch velocity depends so greatly on impact angle. Although I agree with the authors that impact ejecta velocity would be lower at lower impact angles, I do not think ejecta speed is reduced very much at low impact angles. At lower impact angle, less energy is deposited on target, generating a smaller volume of crater cavity. Because of late-stage equivalence in shock propagation, the velocity-mass relation for slowest ejecta component should not depend so much on impact angle. The average ejection velocity for a fix mass of ejecta may be reduced at lower impact angles measured from the horizontal, but the ejection velocity for ejecta mass scaled with crater cavity volume would stay approximately the same because of the late-stage equivalence. Thus, I find mentioning about impact angle effect inappropriate here.

The revised manuscript reads:
"(iii) the current understanding22 is that little mass should be retained by small asteroids from crater ejecta produced by vertical impacts on rocks".
This sentence is emphasizing the impact angle, but what ref. 22 is claiming is the effect of rock strength and porosity. Ref. 22 found that ejection velocity becomes comparable to the escape velocities (<1 m/s) of Bennu and Ryugu for high-porous targets even for vertical impacts. Thus, the above sentence could be revised to something like the following:
"(iii) the current understanding22 is that little mass should be retained by small asteroids from crater ejecta produced by vertical impacts on low-porosity rocks".

Specific comments:
I read each of the authors' responses carefully and found no further needs for revision. Great job!

**Author Rebuttals to First Revision:**

We thank both reviewers and the editor of Nature for providing us with comments and suggestions to improve our manuscript. We believe that we implemented all of them, as detailed in the following.

Please find in the following our detailed answers point by point. **To ease the reading of this reply letter, the reviewers' comments are in *italic*, while our responses are indented in plain text.**

*Referee #1 (Remarks to the Author):*

*Almost ready to go, I only have three minor comments:*

*Line 102 I think should read "consistent with" rather than "consistently with"*

*Reference 37: the wrong journal is cited, it is Chemie der Erde not Geochemistry (the rest of the reference is correct)*

*Caption to Figure 2 seems to be missing a word, should read "Porosity of most of Bennu's..."*

*-- Guy Consolmagno*

> We thank Dr. Consolmagno for the comments, which we implemented.

*Referee #2 (Remarks to the Author):*

*General Comment*

*I have read the revised manuscript and rebuttal letter and found that the authors did a great job revising the manuscript. I also like to indicate that some of my criticism was based on my misunderstanding about the mini-crater observations by Ballouz and that the authors explained very well. Now I really endorse the contents of this manuscript and support publication in Nature. I have* *only one minor recommendation for further revision of the manuscript. That is the effect of impact angle discussed in "(d) Ejection velocity vs. escape velocity". Nevertheless, this disagreement does not affect the main conclusions of this study. This should not stop the publication process.*

We thank the referee for the comments and the endorsement.

*>> The argument does not appear to be very robust. There is no discussion on*

*>> corroborating evidence to support this argument. Because OSIRIS-REx carries so*

*>> many instruments and conducted such a high-resolution imaging observations, I*

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*>> necessary for publication.*

*> We agree that the presentation of corroborating evidence to support Fig. 2 of the*

*> submitted version (now Fig. 3) other than Fig. 1 is necessary. Indeed, we provide*

*> supporting evidence obtained from analysis of instruments (PolyCam and OLA) that*

*> are independent from the OSIRIS-REx Thermal Emission Spectrometer (OTES). In*

*> particular:*

*I really appreciate the added evidence to support difference in d/D ratios of the mini-craters and discussion in the revised manuscript. I find this morphometric evidence to support the difference in response to meteoritic impacts between two types of boulders very convincing, mostly because the nature of this evidence is really independent of the data presented in Figure 1.*

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We are glad that we successfully addressed this concern.

*(a) Porosity-driven frustration of impact fracturing 1.*

*I appreciate the authors' revision on the argument on the ejecta generation and attempt for quantitative assessment. I had an impression that the authors are considering that classic impact ejecta launched upward from the crater cavity created from meteoroidal collisions as the main contributor to the correlation between regolith abundance and thermal inertia. After the revision, however, it is clear that authors are also considering in-situ fragmentation due to meteoroidal impacts. The observation that Bennu's mini-craters rarely have spalls also helps this argument.*

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We are glad that we successfully addressed this concern.

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*Good to see the suggestion has been adopted.*

We thank the referee for the suggestion which improved the manuscript.

*(c) Correlation between thermal inertia and surface abundance of fine regolith*

*Thank you for pointing out my misunderstanding. I also found the revised text will prevent misunderstanding similar to mine.*

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*I found that the authors did a good job resolving the concerns I had about impact ejecta velocity by revising this paragraph. I also agree with the argument about the thermal fatigue regolith formation based on the observation that low boulder strengths estimates on Bennu and Ryugu.*

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*"(iii) the current understanding22 is that little mass should be retained by small asteroids from crater ejecta produced by vertical impacts on low-porosity rocks".*

We thank the reviewer for the comment. We agree to edit the paper following what has been suggested and remove the emphasis on the impact angle. The quoted sentence now reads (lines 145–147): "(iii) the current understanding[22] is that little mass should be retained by small asteroids from crater ejecta produced by impacts on low-porosity rocks."

*Specific comments:*

*I read each of the authors' responses carefully and found no further needs for revision. Great job!*

Thanks!