Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The 1T-TaS2 layered transition metal dichalcogenide is an ideal platform for studying the Mott insulator-metal transition. In this work, the authors find an intrinsic strain-rich area and use STM and STS techniques to reveal the electronic states tuned by strain. They find two types of strain: the mosaic state with several domain walls and the corrugated surface. The closing of the Mott gap and the emergence of V-shaped metallic state are observed in both areas. They combine different theories to explain this transition, including the ratio of Coulomb repulsion U to bandwidth W, the two-orbital-texture theory, and interlayer-coupling effect. This work provides a new tuning method to induce the Mott insulator-metal transition and sheds new light on understanding its mechanism. The results are solid and the paper is well written. I recommend the publication of the paper in Communications Physics after some minor revisions. Below are some detailed comments.

1. Figure 1 and Figure S2 show the temperature-dependent topography of the domain walls. It seems the CDW cluster distribution on each domain wall is not consistent, although most of the domain walls are preserved. Also, the corrugation or height distribution inside each domain changes with temperature. This may indicate the electronic structure difference at different temperature. Does it mean the strain also changes with temperature as well? Can the representative spectra remain the same during warm up? More description of the electronic structure at different temperature will be helpful.

2. The zero-biased dI/dV map in Fig. 2b is very intricate, displaying more features than those discussed in this manuscript. Inside each domain, the CCDW structure is uniform but the DOS at zero energy is not uniform with stripes and defects. It seems the Mott insulator gap is not the typical one as in Fig. 1c with clean U-shape gap. Is there any explanation for that?

3. In Fig. 3e, there are some other bright regions (at the bottom of the image) which have non-zero DOS other than the groove, and they lie on both the ridge and the groove. Does the spectral evolution follow the same trend there?

Reviewer #2 (Remarks to the Author):

In this paper the authors report a strain induced closing of the Mott gap in TaS2. My comments are below.

1) The authors find regions that look corrugated in the topography and deduce that there must be strain. However, they have no experimental evidence of strain (`Without an atomic resolution in this experiment, we can not make a quantitative analysis of the strain based on precise determination of atomic displacement'.

So I don't see how they can assume that strain is primarily responsible for the changes in spectra. The premise of their paper seems weak.

2) I would remove references to the cuprates while discussing the V shaped gap (see paragraph below). As the Mott gap closes its natural for the spectra to look V shaped. The cuprate pseudogap phenomenology is rich and complex and a simple coincidence of gap shapes is not sufficient to make any analogy whatsoever.

For spectra at positions marked from '2' to '5', the differential conductance at -200 mV decreases, the Mott gap is suppressed to a V-shaped gap, and the V-shaped gap develops together with a

finite DOS at zero bias (Fermi energy EF). Although it is not a superconducting state, this V-shaped spectrum is similar to the pseudogap spectrum in cuprate superconductors [33]. For spectra from '1' to '5', the spectrum evolution is reminiscent of how the Mott gap collapses and evolves to a pseudogap state in cuprate superconductors [8].

3) The authors speculate a lot without experimental evidence or theory to support their speculations. For example they say: `The mosaic domains and related complex electronic states are speculated to be induced by the intrinsic strain'. On what basis do they say this?

4) The authors say: As shown in Fig. 4, the bandwidth of LHB is consistently larger than that of UHB, indicating a higher hopping constant t for LHB (t is proportional to W). However, as the spectra change the shape becomes much more difficult to fit with a gaussian. So this statement is not validated by the data,

In general, I find the paper and analysis to be hasty with too much speculation. The quality of the data and the data in general is not sufficient to support their claims. The authors need to do more careful analysis or weaken their claims.

Reviewer #3 (Remarks to the Author):

The authors use STM to explore the CDW phase of 1T-TaS2. They observe a mosaic state of multiple domains near regions where the sample shows a large ridge in the topography, and they attribute the presence of these multiple domains to inhomogeneous strain arising from the changing topographic features. They observe that these domains are stable with increases in temperature, unlike previous reports of similar domains created by pulsed voltage. Results are clearly presented and the observation of the presence and absence of Mott insulating states in different CDW domains should be interesting to the community of researchers working on Mott insulators and 2D materials. However, I think that the attribution of the observed domains to strain, which is the central focus of the paper, is mostly speculative, and some other claims would be stronger if they were explained in more detail.

1. Can the authors rule out other explanations for the mosaic state bedsides strain, such as stacking, defects, edge states, local changes to screening? For example, is it possible to see if the size of the CDW superlattice changes in the different domains? Can the strain in the corrugated region be mapped to the strain in the mosaic region?

2. Can the authors expand the discussion of the different between the Mott insulating domain and the other domains in the mosaic state? What might cause this particular domain to be a Mott insulator when the others are not?

3. I do not understand the purpose of the comparison of the evolution of features 1-5 in Fig. 2C to the evolution of the pseudogap spectrum in cuprates the state in 1T-TaS2 is not a superconducting state. Can the authors elaborate more on this point? Do they expect a transition to a superconducting state at higher strain or some other condition?

I recommend that this paper can be accepted for publication after revision if the authors can address at least some of the questions above in greater detail.

Reply to reviewers:

We thank all the reviewers for their detailed comments on our manuscript. Our replies are in blue color and the original comments of the three reviewers are in black color.

Reviewer #1 (Remarks to the Author):

The 1T-TaS2 layered transition metal dichalcogenide is an ideal platform for studying the Mott insulator-metal transition. In this work, the authors find an intrinsic strain-rich area and use STM and STS techniques to reveal the electronic states tuned by strain. They find two types of strain: the mosaic state with several domain walls and the corrugated surface. The closing of the Mott gap and the emergence of V-shaped metallic state are observed in both areas. They combine different theories to explain this transition, including the ratio of Coulomb repulsion U to bandwidth W, the two-orbital-texture theory, and interlayer-coupling effect. This work provides a new tuning method to induce the Mott insulator-metal transition and sheds new light on understanding its mechanism. The results are solid and the paper is well written. I recommend the publication of the paper in Communications Physics after some minor revisions. Below are some detailed comments.

1. Figure 1 and Figure S2 show the temperature-dependent topography of the domain walls. It seems the CDW cluster distribution on each domain wall is not consistent, although most of the domain walls are preserved. Also, the corrugation or height distribution inside each domain changes with temperature. This may indicate the electronic structure difference at different temperature. Does it mean the strain also changes with temperature as well? Can the representative spectra remain the same during warm up? More description of the electronic structure at different temperature will be helpful.

We are sorry for making this confusion. In the revised manuscript, we replaced Fig. 1e and Fig. S2(g) with a clearer topography under a closer tip sample distance. We also added the corresponding current maps in Fig. S2 which show clearer domain walls. In the previous version, the discrepancy of the height inside domains in Fig. S2 is due to the different method of subtracting background in Fig. S2(f) and Fig. S2(g). All the figures in Fig. 1 and Fig. S2 were revised using the same method of subtracting background, and were rescaled by the same color bar.

In the revised Fig. S2, we can observe that the domain walls are stable during the thermal cycle. For example, Fig. S2(e) (40 K) and Fig. S2(g) (back to 4.5 K) are taken under a closer junction with clearer CDW clusters. The domain walls are the same at these two different temperatures.

The height distribution in each domain is also observed to be nearly unchanged. A bright feature in Fig. S2f is due to that the tip is not fully released at this temperature. In Fig. S3 of the revised manuscript, we show series of representative dI/dV curves at different temperatures. For two positions (around labeled points in Fig. S2), the dI/dV curves are nearly unchanged at different temperatures (Fig. S3).

A strain can tune electronic states in different forms. In the case of the corrugation, the spatial height modulation indicates an inhomogeneous compressive or tensile strain. The electronic state is correspondingly changed. In the case of the mosaic state, the spatial height modulation is rather weak in each domain. The electronic state of materials can be tuned by a relatively homogeneous global strain. For example, in the published literature [PNAS 115, 6986 (2018)], the strain can tune the CDW phase of $NbSe_2$ while preserves the overall flat atomic plane. The strain effect in the mosaic state may also be a global strain, which is easy to be found close to a step edge in both our experiment and the example in [PNAS 115, 6986 (2018)].

2. The zero-biased dI/dV map in Fig. 2b is very intricate, displaying more features than those discussed in this manuscript. Inside each domain, the CCDW structure is uniform but the DOS at zero energy is not uniform with stripes and defects. It seems the Mott insulator gap is not the typical one as in Fig. 1c with clean U-shape gap. Is there any explanation for that?

We thank reviewer #1 for pointing out this question. We rechecked and discussed the electronic state of the mosaic state in more detail in the revised manuscript. Some of the bright spots in Fig. 2b are due to the CDW impurities like missing or distorted David-stars, which are more discernable in a topography under a negative bias voltage (as shown in the comparison between Figs. S8a and S8b). Other bright spots in Fig. 2b always aggregate at the step edge and the edge of each domain. There may be a mechanism that leads to trapped carriers at the edges of domains. Except for the impurity sites and the edge sites, the DOS at the clean area of each domain is relatively homogeneous, as shown in the revised Fig. 2d.

The stripe like conductance is possibly related with a temporary and slight change of the tip. We feel sorry for making this confusion and hope it doesn't affect our main conclusion.

From our experience, the delicate shape of the Mott-gap spectrum is material and tip dependent. Figure 1c and Fig. 2c were taken at different samples by different tips, and exhibits some difference in the Mott-gap spectrum. We also notice that the Mott gap determined by different STM groups exhibit similar difference in published literatures [Qiao et al. Phys. Rev. X 7, 041054 (2017), Ma, et al. Nat. Commun. 7, 10956 (2016), Cho, et al. Nat. Commun. 7, 10453 (2016), Cho, et al. Phys. Rev. B 92, 085132 (2015)]. With the same sample and tip, the Mott-gap spectrum in the mosaic insulating domain should be similar to the clean-area spectrum.

3. In Fig. 3e, there are some other bright regions (at the bottom of the image) which have non-zero DOS other than the groove, and they lie on both the ridge and the groove. Does the spectral evolution follow the same trend there?

The metallic state at the left bottom corner in Fig. 3e may be due to another depression along this direction. In Fig. 3a, there is a large depression at the left bottom corner, which may indicate a complex trough there. Because the area for this special metallic state is small, we cannot extract a line cut to see how the Mott gap evolves across this position.

Besides the corrugation analyzed in Fig. 3, we also find a corrugation in a different area in the

same sample. The Mott gap collapse across this corrugation is shown in Fig. S6 of the revised manuscript. We further conducted experiments on the samples glued on organic-glass substrates. The organic glass has a large thermal expansion coefficient. A compressive strain is expected to act on the sample at low temperature. We observed similar corrugations and electronic behavior in the "strained samples" (Fig. S8 in the revised manuscript).

From all present datasets, we observe that the precise position for the Mott-gap collapse cannot be simply concluded from the height profile of corrugations. The atomic-resolved topography is required for the microscopic analysis of the strain distribution in a corrugation, which is however technically challenging and beyond the scope of this work. We hope that the data presented here is enough to claim that strain could tune the electronic states of 1*T*-TaS_2.

Reviewer #2 (Remarks to the Author):

In this paper the authors report a strain induced closing of the Mott gap in TaS2. My comments are below.

1) The authors find regions that look corrugated in the topography and deduce that there must be strain. However, they have no experimental evidence of strain (`Without an atomic resolution in this experiment, we cannot make a quantitative analysis of the strain based on precise determination of atomic displacement'.

So I don't see how they can assume that strain is primarily responsible for the changes in spectra. The premise of their paper seems weak.

We fully understand the reviewer's concern. In Fig. R1, we plot the height profile along the white arrow in Fig. 3a under two different bias voltages. The rather small ups and downs indicate the modulation induced by the CDW lattice. The large ups and downs indicate the spatial modulation of the atomic plane. Although the amplitude of the small modulation induced by the CDW lattice is weakened under V_b = 1 V, the large spatial modulation remains no change under different bias voltages. This suggests that the corrugation we observed is not an electronic behavior but a real change of the atomic plane. A deviation from a flat atomic plane would naturally cause strain. The height profiles under different V_b are similarly compared for a ripple modulation in Bi_2Te_3 without atomic resolution, which suggests a strain induced phenomenon [Nat. Commun. 3, 1158 (2012)].

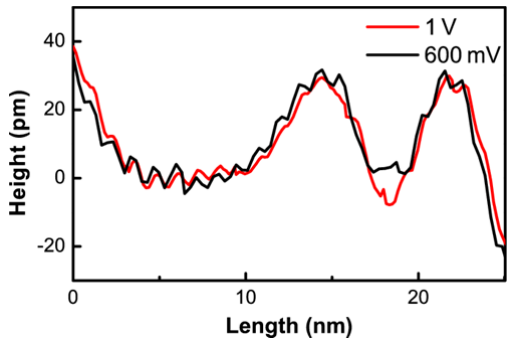

Figure R1. Height profiles along the white arrow in Fig. 3a under V_b = 1 V (red curve) and V_b = 600 mV (black curve).

An atomic resolved topography at the corrugation is surely desired for quantitative analysis of the strain. In our experiment, we occasionally obtained atomic-resolved images if the tip condition is appropriate. For most of the time, it is very challenging for us to control the tip and obtain an atomic resolution in this material (1*T*-TaS_2). Because the topography with strain is more complex, it would be harder to achieve this goal. Further experiment on the atomic resolved topography will be of great help for the understanding of strain tuned electronic behavior.

Alternatively, we intentionally introduce strain to a sample and search the corrugation in this "strained sample". We glue the single crystal sample of 1*T*-TaS_2 (3 mm x 3 mm x 0.1 mm) on the 5 mm x 5 mm x 1 mm organic-glass substrates following the similar method reported in Gao, S. et al. PNAS 115, 6986-6990 (2018). The organic glass has a large thermal expansion coefficient. A compressive strain is expected to act on the sample when the temperature is lowered. We find both the mosaic state pattern and the corrugation in the "strained sample". We have tested three samples and the corrugations are found in all of them. In Fig. S8 of the revised manuscript, we show a corrugation found in the strained sample. The corrugation is again clarified to be a spatial modulation by comparing the height profile under positive and negative bias voltages (Fig. S8c).

2) I would remove references to the cuprates while discussing the V shaped gap (see paragraph below). As the Mott gap closes its natural for the spectra to look V shaped. The cuprate pseudogap phenomenology is rich and complex and a simple coincidence of gap shapes is not sufficient to make any analogy whatsoever.

For spectra at positions marked from '2' to '5', the differential conductance at -200 mV decreases, the Mott gap is suppressed to a V-shaped gap, and the V-shaped gap develops together with a finite DOS at zero bias (Fermi energy EF). Although it is not a superconducting state, this V-shaped spectrum is similar to the pseudogap spectrum in cuprate superconductors [33]. For spectra from '1' to '5', the spectrum evolution is reminiscent of how the Mott gap collapses and evolves to a pseudogap state in cuprate superconductors [8].

We agree with the reviewer. The pseudogap in cuprates is rich and complex. In the revised manuscript, we soft our tone and mainly focus on the related 1*T*-TaS_2 system. In a Cu intercalated 1*T*-TaS_2, a metallic V-shaped gap is found to gradually disappear at a high temperature [Phys. Rev.

Lett. 112, 206402 (2014)]. In 1*T*-$TaS_{2-x}Se_x$ with isovalent substitution of S with Se, a V-shaped gap is also observed [Phys. Rev. X 7, 041054 (2017)].

3) The authors speculate a lot without experimental evidence or theory to support their speculations. For example, they say: `The mosaic domains and related complex electronic states are speculated to be induced by the intrinsic strain'. On what basis do they say this?

As the reply for the 1st question, we mainly conducted additional experiment on the "strained sample" glued on an organic glass substrate. The mosaic state is also found in the strained sample, as shown in Fig. S7 in the revised manuscript. This is a direct evidence for the strain induced mosaic state.

The metallic mosaic state has been caused by external perturbations such as super-cooled process [Nat. Commun. 7, 10956 (2016)], or a voltage pulse [Nat. Commun. 7, 10453 (2016), Nat. Commun. 7, 10956 (2016)]. Following we also provide some reasons because of which we speculate that the mosaic state is induced by the intrinsic strain.

1. We gradually cooled all the samples and inserted the samples into STM head. Most samples show flat area as in Fig. 1. The same cooling process for this sample cannot induce such a mosaic state by the super-cooling.
2. No voltage pulse was applied to the sample.
3. The mosaic state is rather stable than the super-cooled process induced mosaic state and pulse induced mosaic state, indicating a different origin. In fact, the melting of the domain walls should conquer the barrier with enough energy. Strain is a natural thought to pin down the domain wall and makes the mosaic state more stable.
4. The FOV in the mosaic state is surrounded by troughs and islands. The corrugation not far from this area is also analyzed. The complex topography is abnormal compared with the typical flat area. It is reasonable to speculate the strain as a possible candidate.

4) The authors say: As shown in Fig. 4, the bandwidth of LHB is consistently larger than that of UHB, indicating a higher hopping constant t for LHB (t is proportional to W). However, as the spectra change the shape becomes much more difficult to fit with a gaussian. So this statement is not validated by the data,

In Fig. S5b of the revised manuscript, we show six representative fitting to the Hubbard bands. The locations of the spectra are indicated by the red arrows in Fig. S5a. The Hubbard bands are reasonably fitted by the Gaussian function even when it approaches the corrugation (see point spectra of 4 and 5 in Fig. S5b).

Here we also show the whole fitting results in Fig. R2. For spectra far from the corrugation, the Gaussian function fits well for the Hubbard bands. Around the center of the Mott-gap collapse, some of the spectra cannot be fitted well by the Gaussian function. To avoid the possible confusion by the fitting, the result of spectra 38 to 44 is not shown in the main text. The quantitative results show a larger bandwidth for the LHB. In fact, the bandwidth of the UHB is already smaller than that of LHB far away from the corrugation. As the position approaches the

corrugation, the valence band moves to the LHB and the bandwidth of LHB is even larger. From a qualitative perspective, the bandwidth of LHB is also consistently lager than that of UHB.

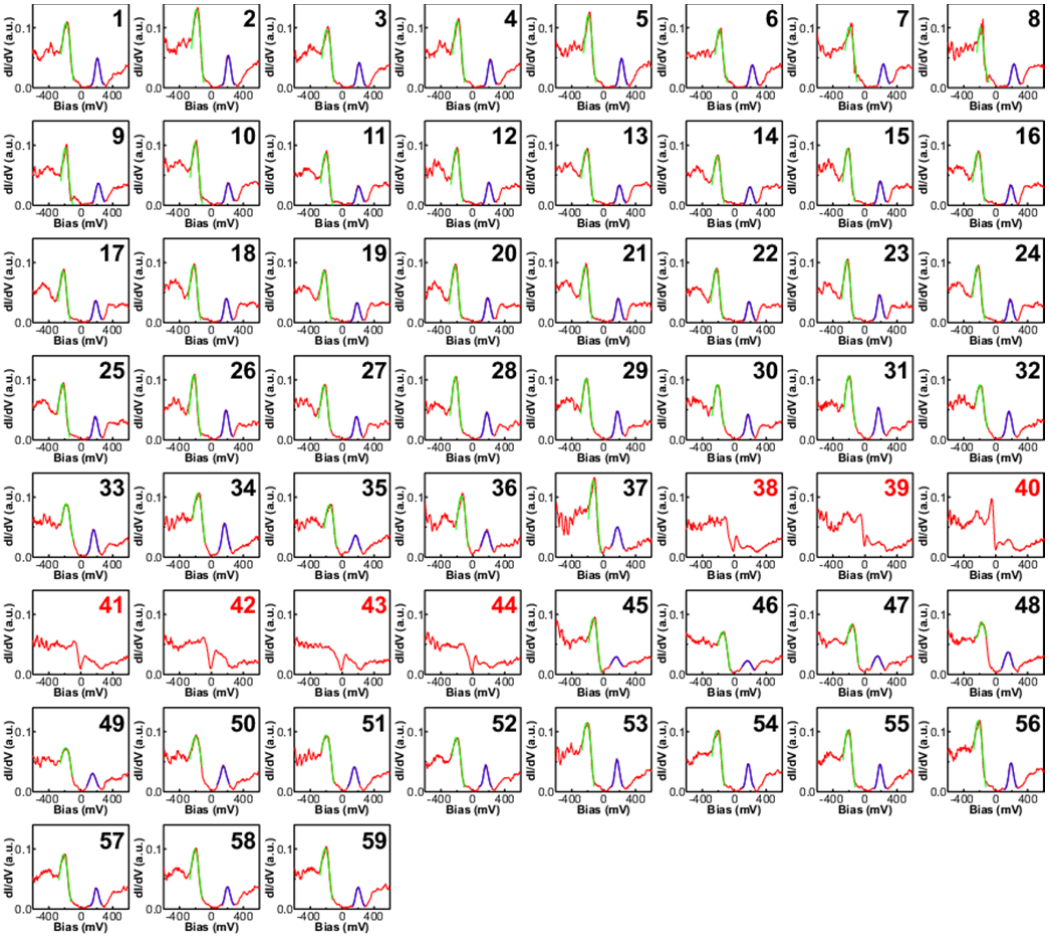

Figure R2. Fitting of the Hubbard bands by a Gaussian function. The raw *dI*/*dV* curves are indicated by the red curves. The fitting results of the UHB and LHB are represented by the blue and green curves, respectively. The index in each figure is ordered by its distance from the original point. The fittings of spectra 38 to 44 are not well and correspondingly not shown.

In general, I find the paper and analysis to be hasty with too much speculation. The quality of the data and the data in general is not sufficient to support their claims. The authors need to do more careful analysis or weaken their claims.

Reviewer #3 (Remarks to the Author):

The authors use STM to explore the CDW phase of 1T-TaS2. They observe a mosaic state of multiple domains near regions where the sample shows a large ridge in the topography, and they attribute the presence of these multiple domains to inhomogeneous strain arising from the changing topographic features. They observe that these domains are stable with increases in temperature, unlike previous reports of similar domains created by pulsed voltage. Results are clearly presented and the observation of the presence and absence of Mott insulating states in

different CDW domains should be interesting to the community of researchers working on Mott insulators and 2D materials. However, I think that the attribution of the observed domains to strain, which is the central focus of the paper, is mostly speculative, and some other claims would be stronger if they were explained in more detail.

We thank reviewer #3 for the overall positive comments on our manuscript. We conducted additional experiment on the "strained sample" glued on an organic-glass substrate. Similar mosaic state was found in the strained sample (Fig. S7 of the revised manuscript), which provides a direct evidence that the mosaic domain in our experiment is caused by the strain. In the revised manuscript, we also discussed the electronic states of the mosaic state in more detail.

1. Can the authors rule out other explanations for the mosaic state bedsides strain, such as stacking, defects, edge states, local changes to screening? For example, is it possible to see if the size of the CDW superlattice changes in the different domains? Can the strain in the corrugated region be mapped to the strain in the mosaic region?

In the reply to reviewer #2, we rule out some possible reasons such as super cool process and voltage pulse. In the newly found mosaic state in the strained sample (Fig. S7), the mosaic domains were found in a place with complex morphology, which shares the similar behavior as the mosaic state in the main text. The strain-induced mosaic state can be further explored with more datasets. During our experiment on the strained sample, we found that the organic glass degassed upon warming, which contaminated the vacuum chamber and limited the data-taking. A more proper substrate should be selected later for the detailed exploration.

In Fig. R3, we show five representative height profiles in different domains. There is no obvious size change of the CDW lattice among these domains (within our resolution). Although the size of the CDW lattice keeps the same, the electronic state varies at different domains, as shown in Fig. 2c for the corresponding dI/dV curves.

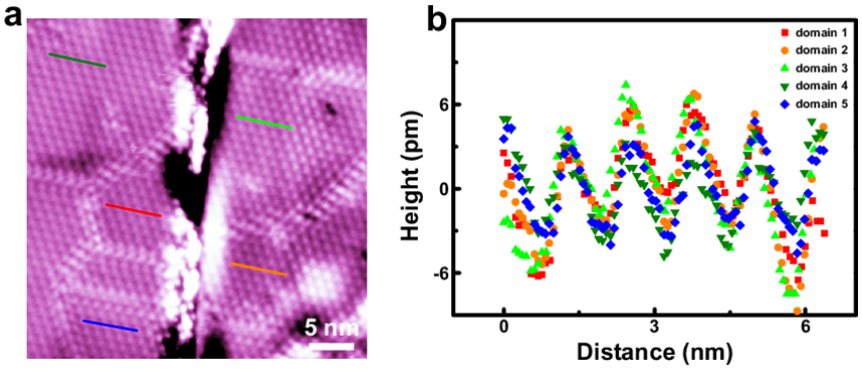

Figure R3. **a** The topography of the mosaic state at 4.5 K reproduced from Fig. 1e. **b** The height profiles corresponding to the five line cuts in **a**. Colors are consistent with different domains marked in Fig. 2a and Fig. 2b.

The simultaneous observation of the mosaic domain and the corrugation in the main text as well as in the "strained sample" suggests the strain as the common origin. A strain can tune

electronic states in different forms. The inhomogeneous strain at the corrugation is evidenced from the large spatial modulation of the atomic plane. A global strain may also change the electronic state without forming a corrugation. An example is shown in PNAS 115, 6986 (2018). The surface in the mosaic state is relatively flat. In the revised section of Discussion, we carefully discussed the different strain effect on the mosaic state and corrugated area.

2. Can the authors expand the discussion of the different between the Mott insulating domain and the other domains in the mosaic state? What might cause this particular domain to be a Mott insulator when the others are not?

The Mott insulating domain inside the metallic mosaic domain is not yet fully understood. In the published literatures [Nat. Commun. 7, 10453 (2016), Nat. Commun. 7, 10956 (2016)], domains surrounded by the top layer and inner layer domain walls can be insulating. It provides strong evidence for inter layer stacking. The insulating domain in Fig. 2 is at the edge of a step, the stacking mechanism may also be applied in our case.

3. I do not understand the purpose of the comparison of the evolution of features 1-5 in Fig. 2C to the evolution of the pseudogap spectrum in cuprates the state in 1T-TaS2 is not a superconducting state. Can the authors elaborate more on this point? Do they expect a transition to a superconducting state at higher strain or some other condition?

As our reply to reviewer #2, we soft our tone for the analogy with the pseudogap in cuprates. We mainly focus on the related 1*T*-TaS_2 system. A V-shaped gap is generally observed in Cu intercalated and isovalent Se doped 1*T*-TaS_2 [Phys. Rev. Lett. 112, 206402 (2014), Phys. Rev. X 7, 041054 (2017)]. The V- shaped gap gradually disappears at a high temperature in Cu intercalated 1*T*-TaS_2.

From our present data and experimental conditions, we cannot judge whether the observed V-shaped gap is a superconducting state at a lower temperature. It is helpful to explore the possible superconducting state at a lower temperature in the future.

I recommend that this paper can be accepted for publication after revision if the authors can address at least some of the questions above in greater detail

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

All my concerns have been satisfactorily addressed in the response and revised manuscript. Now the paper is ready for publication.

Reviewer #2 (Remarks to the Author):

The authors report `a strain induced Mott gap collapse in TaS2' (the title of their paper).

I am afraid I find no experimental evidence whatsoever that strain is responsible for the changes in the local density of states seen by them.

The authors find corrugations in the topography. They then measure the spectra which show changes across these corrugations. The authors then speculate that the spectra are different due to strain. But this is PURE speculation. The only indirect evidence for strain in these regions is that the topography shows height variations. But these variations are small and could easily be cause by the density of states changes seen by them! So the authors have no proof what so ever that strain is involved.

TaS2 is a complex material and as seen in many previous papers, even CDW domain walls or stacking differences can create Mott collapse. The authors have no way of distinguishing between these different reasons. In fact, the authors seem to be simply picking one possible explanation and leaving the others aside with no experimental basis.

Another point the authors make is that they find the same mosaic patters on strained substates. But all this proves is that the Mosaics are generated as a response to strain. It does not mean that the Mott gap is collapsing due to strain.

All in all, the authors claims are over blown and not substantiated by the data.

Reviewer #3 (Remarks to the Author):

I am satisfied with the changes the authors have made and feel that the additional experiments conducted have strengthened the claims in the manuscript. Consequently, I recommend that the paper be accepted for publication.

Reply to reviewers:

Our replies are in blue color and the original comments are in black color.

Reviewer #1 (Remarks to the Author):

All my concerns have been satisfactorily addressed in the response and revised manuscript. Now the paper is ready for publication.

We thank reviewer #1 for the supporting of publication in Communications Physics.

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The authors report `a strain induced Mott gap collapse in TaS2' (the title of their paper).

I am afraid I find no experimental evidence whatsoever that strain is responsible for the changes in the local density of states seen by them.

We agree with the reviewer's comment. In the revised manuscript, we mainly soften the relation between strain and the underlying mechanism to control the Mott gap collapse. As the reviewer said in the following comment "the Mosaics are generated as a response to strain", we modify our expression to that the strain can induce corrugations and mosaic state in 1*T*-TaS_2, and the Mott gap collapse around the corrugation and mosaic state is studied and analyzed. The possible roles of stain in the mechanism are only speculated and discussed together with other possible mechanisms. The title is changed to "Possible strain induced Mott gap collapse in 1*T*-TaS_2".

The authors find corrugations in the topography. They then measure the spectra which show changes across these corrugations. The authors then speculate that the spectra are different due to strain. But this is PURE speculation. The only indirect evidence for strain in these regions is that the topography shows height variations. But these variations are small and could easily be cause by the density of states changes seen by them! So the authors have no proof what so ever that strain is involved.

In the revised manuscript, we modify the expression that the strain induces corrugations, around which the Mott-gap collapse is observed and analyzed. The corrugation is also confirmed in the "strained sample". In the following comment, the referee agrees that the Mosaics are generated as a response to strain. Similarly, the corrugations are also generated as a response to strain in the "strained sample". In the revised manuscript, we add extra information to support this observation. "We also conducted the experiment on samples glued on the SiO_2 (0001) substrate, which has a rather small thermal expansion coefficient and is expected to bring a tensile strain to the glued sample at low temperatures. Instead of corrugations, cracks are the dominant features on the surface for samples glued on the SiO_2 (0001) substrate."

The change of electronic density of states can be incorporated in the topographic image. This electronic information can be distinguished by checking topographies under different bias voltages. We have demonstrated that the corrugation is not an electronic behavior but a spatial modulation.

Please see Fig. 8c in Supplementary and Fig. R1 in the former rebuttal letter. Although the height variation is small (tens of picometers), it is a reasonable value as that of other strain-induced corrugations [PNAS 105, 3203-3208 (2008), Nat. Commun. 3, 1158 (2012), Nat. Nanotechnol. 10, 849-853 (2015)].

To our knowledge, a spatial modulated corrugation (or a deviation from the structure it should be) will naturally involve strain. However, smoking gun evidence for quantitative strain and its relation to the corresponding electronic states should de deduced from atomic resolved STM image, which is technically difficult at present stage. We soft our tone that the strain is a possible candidate for the Mott gap collapse at the corrugation.

TaS2 is a complex material and as seen in many previous papers, even CDW domain walls or stacking differences can create Mott collapse. The authors have no way of distinguishing between these different reasons. In fact, the authors seem to be simply picking one possible explanation and leaving the others aside with no experimental basis.

Around the domain wall, the periodic lattice of the Star-of-David (SOD) is destructed. The relative translational shift of SOD is well studied for different types of domain walls. Around corrugations in our experiment, there is a single domain without any domain walls, which proves that this Mott-gap collapse is a new phenomenon different from that induced by the domain-wall. We have also discussed the differences of Mott gap collapse between the corrugation and domain walls (please see Supplementary Note2). The Mott gap suffers a sudden collapse at the domain walls while it collapses smoothly at the corrugation.

As for the stacking differences between neighboring layers, we have no evidence to exclude this possibility because STM cannot resolve the inner layer SOD at the corrugation. The stacking differences would cause an entire domain (or subdomain surrounded by domain walls at different layers) to be insulating or metallic [Nat. Commun. 7, 10453 (2016), Nat. Commun. 7, 10956 (2016)]. We don’t see any domain wall in Fig. 3a but there are both insulating and metallic states in this area.

Although we distinguish the Mott-gap collapse around corrugations from that induced by domain wall and stacking layer, we don’t have a quantitative evidence to relate the strain with the electronic state modification. The expression is modified to that stain possibly induces the Mott gap collapse at the corrugation.

Another point the authors make is that they find the same mosaic patters on strained substates. But all this proves is that the Mosaics are generated as a response to strain. It does not mean that the Mott gap is collapsing due to strain.

The mosaic state is complex with various electronic states, and the mechanism is not yet fully understood. In our experiment, the mosaic state can be induced by strain, as the referee has suggested. In the revised manuscript, we soft our tone for the relation between strain and the Mott gap collapse at the mosaic state. We strengthen the strain as a new tuning knob to induce this

mosaic state. The possible roles of strain are carefully discussed together with other possible mechanisms.

All in all, the authors claims are over blown and not substantiated by the data.

Reviewer #3 (Remarks to the Author):

I am satisfied with the changes the authors have made and feel that the additional experiments conducted have strengthened the claims in the manuscript. Consequently, I recommend that the paper be accepted for publication.

We thank reviewer #3 for the recommendation of publication.