nature portfolio

Peer Review File

Atomically Resolved Edges and Defects in Lead Halide Perovskites

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Referee #1

(Remarks to the Author)
Leveraging the high-speed, event-driven Timepix3 hybrid pixelated direct electron detector, the authors reveal the atomically resolved structure of one of the most beam-sensitive materials, $MAPbI_3$ hybrid halide perovskite, using the 4D-STEM ptychography technique under ultra-low dose conditions of just a few tens of electrons per square angstrom. Notably, the authors integrate the dose fractionation technique, widely used in CryoEM and HR-TEM, into 4D-STEM ptychography, enabling them to elucidate the dynamics of the more vulnerable edge and defect structures under electron illumination for the first time. The resolved termination configurations of the film edge and defect edge allow the authors to correlate these configurations with the stability of nanosheet and defect edges under electron beam illumination. This, in turn, leads to informed recommendations for optimizing the film synthesis process to enhance performance.
The results demonstrated, for the first time, the exceptional capabilities of 4D-STEM ptychography in delivering atomic-resolution imaging of ultra-beam-sensitive materials, such as hybrid halide perovskites, a long-anticipated achievement (Ref. 1). Even more impressively, its extraordinarily low-dose handling capabilities enable the dynamic study of such beam-sensitive materials. Considering the advantages of 4D-STEM ptychography over other commonly used imaging techniques (HR-TEM, ADF-STEM, iDPC, etc.), including easier image interpretation, better contrast for both heavy and light atoms, greater flexibility in beam focus, and improved tolerance to sample thickness, these results are expected to catalyze research interest within the community.
The paper contains impressive data; however, significant content reorganization/rewriting, and improvements in data presentation quality are necessary before it can be recommended for publication. The authors are requested to address the following questions and comments.
1. The abstract concisely summarizes the key achievements of this paper. However, there is insufficient experimental data to support the claim of “acquisition of atomic-resolution edge and defect structures of perovskite at a dose of 10 $e^-/Å^2$.” The single-frame image with a dose of 13 $e^-/Å^2$ (SFig. 17) is not clear enough to reveal the atomic structure at the edge. While Fig. 3, which is a sum of three frames, does reveal the edge structure, but without dose information. In the same figure, the averaged single frame data shows significantly higher S/N ratio, but this should be mentioned in the claim, as it is an averaged representation. The clear atomic defect structures in the interior region (Fig. 4d, g) are obtained at doses of 127 and 109 $e^-/Å^2$, respectively. Therefore, the authors should either provide additional robust experimental data to support this claim or revise the claim to reflect the presented experimental data. Including a table in the Supplementary Information that summarizes the imaging conditions (including dose) for all the images is recommended.
2. In the conclusion section, two of the claims—“imaging can be achieved with approximately 10 $e^-/Å^2$, a temporal resolution of 0.52 s, and an Abbe spatial resolution of 1.2 Å”—are questionable, as there is no convincing evidence that the 10 $e^-/Å^2$ dose and 1.2 Å resolution occur simultaneously. The 1.1 Å resolution of the bulk structure (Fig. 2) is achieved at a dose of 32 $e^-/Å^2$, and the 1.25 Å resolution at a dose of 10 $e^-/Å^2$ is based on simulated dataset (SFig. 7a, d). There is no specified resolution for the single-frame experimental data at a 10 $e^-/Å^2$ dose (SFig. 11 i, j), possibly due to the high noise level in the FFT. Additionally, the claim of “a temporal resolution of 0.52 s” only appears in the conclusion and should have been mentioned earlier in the imaging section. A rewrite of this section is necessary.
3. The authors provide good references to previously published work. However, to highlight the significance of this paper, it is recommended that the authors comment on a closely relevant paper by M. U. Rothmann et al. (Ref. 2) to demonstrate the advantages and uniqueness of the 4D-STEM ptychography technique. The authors may also consider replicating the

experiment using an annular dark field detector and comparing the results in the supplementary information. In that reference, Rothmann et al. studied a similar hybrid perovskite material ($FAPbI_3$) using low-angle annular dark field (LAADF) STEM technology, employing a standard STEM annular dark field detector with image processing filters under an electron dose as low as 53 $e^-/Å^2$. Although they claimed to have imaged $MAPbI_3$, no relevant images were provided. This naturally raises the question of whether STEM ptychography is necessary for this type of study. Addressing this issue is crucial, as it impacts the perceived importance of your paper.
4. Proper image annotation is missing in this paper. None of the color-coded atomic models are labeled, making it difficult for readers unfamiliar with the structure to interpret the images.
5. For the film edge termination, the authors should include a Zoom-in inset with arrows indicating Pd/I column and MA columns.
6. In Fig. 3C, there is a claim that $MAPbI_3$ transforms into PbI_2, but no obvious supporting evidence is provided. An atomic model showing the lattice similarity along a specific alignment of the two phases (e.g., (1 -1 1) PbI_2 / (001) $MAPbI_3$) should be included to help readers easily verify that the PbI_2 phase has indeed formed as claimed. Additionally, at least one arrow should be used to point to the location where this transformation occurs.
7. In SFig. 19, the arrows indicating the PbI_2 phase formation should be placed more accurately; some are off by about one unit cell, which could confuse readers. Additionally, there is a confusing typo in the caption; "PhI_2" should be corrected to "PbI_2." Throughout the paper, there are several misspellings and grammatical errors that should have been corrected before submission.
8. In Lines 83-84, the authors mention that a focused probe was chosen due to the distinct nature of the sample. Since this is a technology-focused paper, a more detailed explanation is warranted. Are there additional reasons for this choice, such as the possibility that certain algorithms (e.g., SSB, Ref. 3?) may not perform well with defocused probes? Typically, it is believed that a defocused probe is more dose-efficient (Ref. 1), and one of the advantages of STEM ptychography over iDPC is that it does not necessarily require a focused probe. Additionally, creating a focused probe might incur a higher dose cost.
9. The authors could emphasize the low-dose handling advantage of STEM over TEM by referencing SFig. 3 and quoting the TEM results from Ref. 1 at room temperature and Ref. 4 under cryo conditions (-175°C). At room temperature, both SFig. 3 and Ref. 1 show that $MAPbI_3$ begins to degrade at a dose as low as 6 $e^-/Å^2$, whereas under cryo conditions, the critical dose for $MAPbI_3$ is 12 $e^-/Å^2$. Furthermore, if the authors could provide some insight into why this material can tolerate a higher dose in STEM mode compared to TEM mode (even if this is beyond the immediate scope of the paper), it would significantly enhance the value of the paper. Many people believe the opposite is true.
10. As dose is a critical parameter in this study, the authors should describe the specimen finding and beam focusing procedures in detail. Specifically, how much dose does it take to find the sample and focus the beam, and is there any protocol to minimize the dose of pre-imaging? In HR-TEM imaging, pre-HR imaging handling is also crucial, as the sample might be damaged even before imaging begins (Ref. 5). Therefore, providing this information is important for readers.
11. The samples studied in this paper are very thin, only a few nanometers. Conducting an analysis similar to that in Fig. 2 on samples with a thickness of tens of nanometers (e.g., 20 to 50 nm) could demonstrate the robustness of this technique, broaden its applicability, and attract the interest of users who work with thicker samples.
12. The ultra-fast event-driven hybrid detector is a key enabler for this work. The authors are encouraged to briefly explain why they chose this type of Timepix camera over other commonly used cameras, such as the Medipix camera. This information would assist users who are less familiar with these technologies in selecting the appropriate camera for their applications. This discussion could be included in the Supplementary Information.
I am not familiar with the dose calibration using the Timepix camera, so I cannot comment on this aspect.

References
1 K.P. Song et al., Atomic-Resolution Imaging of Halide Perovskites Using Electron Microscopy, Adv. Energy Mater., 10, 1904006 (2020)
2 Rothmann et al., Atomic-Resolution Imaging of Halide Perovskites Using Electron Microscopy, Science, 370, eabb5940 (2020).
3 G. Li et al., 4D-STEM Ptychography for Electron-Beam-Sensitive Materials, ACS Central Science, 8, 1579 (2022)
4 Y. Li et al., Unravelling Degradation Mechanisms and Atomic Structure of Organic-Inorganic Halide Perovskites by Cryo-EM, Joule, 3, 2854 (2019).
5 D. Zhang, et al., Atomic-Resolution Transmission Electron Microscopy of Electron Beam-Sensitive Crystalline Materials, Science, 359, 675 (2018).

Referee #2

(Remarks to the Author)
In this work Yuan and colleagues present a low electron dose study of the edge structure and defects in lead halide perovskite materials. They employ electron ptychography to form atomic resolution images at low electron dose which with resolution greater than the corresponding annular dark field images and show a breakdown of crystallinity via mass loss with increased cumulative dose.

The approach builds on established work in the field for low electron dose ptychographic imaging [e.g. Scientific Reports 9,3919 (2019), Appl. Phys. Lett. 116, 124101 (2020)] and does represent, to the best of my knowledge, the lowest dose example of atomic resolution ptychography. However these materials have previously been successfully imaged at atomic resolution using conventional aberration corrected TEM imaging by Rothmann et al. [Science 370,eabb5940(2020)] all be it at slightly higher electron doses.

In this work Yuan and colleagues successfully image the atomic edge structure of small, thin MAPbI flakes, to the best of my knowledge the first time this has been achieved.

Though this represents a significant achievement I cannot recommend publication of this manuscript in Nature for the following four primary reasons:

1. The quality of the ptychographic reconstructions is significantly poorer than the state of the art in the field. This is to be expected for very low dose experiments but there are clear and obvious deficiencies in the ptychographic reconstructions probably due to phase wrapping which are not addressed at all.

2. The authors primary conclusion is that edge termination and defects have a major impact on the material stability. The experimental evidence presented for different edge terminations is not conclusive and the authors do not determine the structure of specific classes of edge defect and correlate these with stability. The data presented is from a single edge site and a single bulk defect. In order to come to robust conclusions regarding material structure would require numerous examples of each to be presented.

3. There is very little quantitative analysis of bond lengths and no attempt to compare the reconstructed phase images with simulation to validate conclusions regarding atomic structure.

4. The manuscript is peppered with unsubstantiated claims, omissions and inaccurate interpretation of data (detailed below) and the authors draw conclusions which are almost wholly unsupported by the presented experimental data (detailed below).

Detailed comments.

Strictly speaking radiation dose is a measure of energy deposited and has units of Grays (Gy). The correct term for 'number of incident particles' is particle fluence which has units of number of particles per unit area. However the two terms (dose and fluence) are used interchangeably in the literature.

The references are oddly numbered, alphabetically which is not standard for the journal and makes following references more challenging for the reviewer. Please reformat in the journal standard style.

Line 83: "We opted for a scanned focused electron beam due to the distinctive nature of the observed sample, MAPbI3"

What does 'distinctive nature of the observed sample" mean? This isn't a justification for the chosen optical setup.

Line 94: "...and the thickness of similar nanosheets are in the range of 1-3 nm using electron energy loss spectroscopy"

Poor English language. Please provide more information in the SI regarding how the thickness was calculated from the raw spectroscopic data. Specifically what technique was used and what value for inelastic mean free path was choosen and with what justification?

Supplementary Fig 3: Please explain the annotations - orange and blue arrows - in the caption. What was the electron fluence (dose) used in the initial TEM image?

From a single orientation diffraction can you discount a partial tilting of the tetragonal phase from viewing down the [001] direction to viewing down [100] or [010] rather than a transformation to orthorhombic? If this conclusion is based on the literature then this needs to be cited.
What are the measured bond lengths from the diffraction patterns - how do they correspond to literature values from e.g. xray studies?

Line 101:"Despite the lower irradiation intensity of TEM, its continuous plane wave illumination actually results in significantly more cumulative damage. Surprisingly, using an extremely rapidly scanned probe the sample exhibits over 10 times the stability compared to a parallel electron beam. Therefore, choosing a focused probe and fast scanning holds significant potential to extract a greater amount of information before damaging the sample."

"Dose rate" dependence has been investigated in Ultramicroscopy 232 113398 (2022) with theoretical interpretation in Ultramicroscopy 240 113568 (2022). These findings explain the greater damage in plane wave illumination.

Also Figures 3-5 in supplementary information need accompanying text as there is not sufficient information in the main text or in the supplementary figure captions to explain to the reader how the data demonstrates the authors claims that there is less damage with a scanning beam than with the plane wave illumination.

Line 114: "...apart from 1 or 2 pixels,"

There are exactly 2 events shown in fig 1c - though it looks like 4 px counts, the upper event having counts in consecutive horizontal and the lower event counts in consecutive vertical pixels.

I can accept that each discrete event is due to one incident electron - with two px being illuminated due to the non-perfect

modular transfer function of the detector - though for an event based camera like the timepix it should be possible to account and correct for this. This does not align with the statement "a single CBED yields only ~1 e–" (line 113). The displayed frame clearly shows 2 electrons. Calculation of the expected number of events per frame for 0.2 pA and 500ns frame time gives 0.6 electrons per frame and 0.3 pA gives 0.9 electrons per frame. It is possible to accurately calibrate your beam current from your dataset. Either your beam current is not calculated correctly or the diffraction pattern you show is not representative of the data set.

Figure 1: "Color bars are from 0 to 1, and from 0 to 0.0015, respectively."
What are the units of the sum CBED (or PACBED) pattern, why is the scale from 0 to 0.0015. This is not the number of recorded electrons as is shown in the individual CBED pattern. This needs correcting.

Line 120: "While the scattered electron signal beyond 40 mrad is weak, the radial distribution over which the high-angle ADF (HAADF) detector integrates isbroad enough to capture signals from heavy atoms like Pb simultaneously with the 4D-STEM."

What is the inner angle of the ADF detector? I assume it is beyond the outer angle of the timepix camera - this should be stated explicitly.

What is the difference between the intensity in the BFD and that in the (extrapolated) region of the ADF detector from the PACBED image? From this value you can calculate the average number of electrons per unit angle and therefore know how many electrons you would expect to measure per probe position in the ADF detector. I'm surprised that with just two electrons in the BFD you get any signal at all in the ADF - this should be well below the Poisson noise limit - unless you are greatly oversampling in your probe space - what is the probe step size? Please address these comments in the text.

Line 127. "The robustness of non-iterative ptychography to noise allows for phase image reconstruction with minimal signal 20."

Reference 20 [Pennycook TJ, Lupini AR, Yang H, et al (2015) Efficient phase contrast imaging in stem using a pixelated detector. part 1: Experimental demonstration at atomic resolution. Ultramicroscopy 151:160–167] does not show data with minimal counts on the detector - in fact the data presented in this reference is acquired at 30 frames per second, though the beam current is not stated. A better reference is O'Leary et al Appl. Phys. Lett. 116, 124101 (2020) which was the first demonstration of ptychography using 'binary' data.

Line 129: "However, iterative ptychography often struggles with such an extremely low number of electrons per CBED pattern due to the difficulty in guiding the convergence."

Is this statement based on evidence in the literature. If this is based on previous work in the literature please provide a citation. If not then some evidence from your study needs to be presented to support this statement.

Line 139: "Through the iterative and non-iterative phase reconstruction methods, achieving
atomic-level phase and ADF imaging becomes possible."

It is not clear how the ptychographic reconstructions enable atomic resolution ADF imaging. Please provide more detailed information.

Supplementary fig 6:
Subfigures b) and f) show the aligned sum of single frames similar to subfigures a) and e). There are almost no discernible features in a) or e). I'm not convinced you can reasonable align subsequent phase images which are so noisy with no clearly discernible features. Please provide further details on how this was done.
Sub figures c and g) - are these simply summing diffraction patterns, with no alignment? This is not clear in the caption. If this is the case, how are inconsistencies in probe positions on subsequent scans accounted for? Please provide additional information.

Supplementary figure 7:
There are some phase wraps in the reconstructions resulting in the heavier element columns appearing as bright doughnuts with a dark center - this can be corrected for using a standard phase unwrapping algorithm. This problem is seen in all subsequent ePIE reconstructions and makes them significantly more difficult to interpret as well as precluding the possibility of an quantitative phase analysis. Please resolve this issue or at the very least have a detail discussion highlighting the presence, cause and reasons why it can not be resolved.

For c) and f) how are the inconsistencies in probe positions accounted for when the diffraction data is summed? If the global drift / scan distortions are corrected for by comparison with the individual reconstructions there will still be a variable probe 'jitter' within each scan. The standard approach to corrected this is to use a probe position annealing technique (Ultramicroscopy
Volume 120, September 2012, Pages 64-72) - this would not be possible on the summed diffraction data. Please provide more detail on the summed reconstruction process.

"In order to realize the sub-pixel alignment, we increase the probe position sampling by 2 (more is possible, but that would need much more memory to load and process 4D-STEM data). "

What does this mean? Are you collecting addition probe positions (or using additional probe positions from the acquired data)? How/why does this enable sub-pixel alignment. Please provide more detail.

Please show reconstructed probe functions alongside the reconstructed images (or in the supplementary information). It is difficult to assess how realistic the object reconstruct is without being able to examine the probe function.

Line 152: "Whereas the phase images reveal the MA+ columns..."

It is not clear that this is the case - please mark the location of the MA+ on the figure and provide a description in the caption.

Line 153: "The Fourier transforms (FT) of the phase images..."

Strictly speaking these are two dimensional power spectrums which are calculated using a Fourier transform.

Figure 2:
It looks like the ADF image has a significantly smaller pixel size that the ptychographic reconstructions. It should be possible to bin (sum pool) the ADF image to the same px size as the ptychography reconstructions. This will improve the signal to noise of the ADF image. With the addition of common smoothing techniques (e.g. Gaussian kernel filter, two-dimensional Savitsky-Golay etc) it may be possible to improve the quality of the ADF image to similar to the ptychographic reconstructions. Please provide this comparison to convince the reader that the ptychographic reconstructions really do give an improvement in signal to noise and resolution.

Again the ePIE reconstructions show what looks like phase wrapping at the centre of the heavy atom columns (as do all subsequent ePIE reconstructions).

The atomic position markers in e) are poorly placed and miss the atomic columns on the bottom row. They are also not equally spaced - particularly the bottom row.
Subfigure h), is this measure of information transfer a reliable gauge of usable image resolution? Please comment. What is the probe step size in this data? It is common for their to be features in the 2D power spectrum corresponding to probe step size. Is this the full power spectrum or has it been cropped? If this is the full power spectrum then there is information beyond Nyquist frequency which should not be possible. If cropped please state that this has been done. There seems to be information beyond the 1.1A marked on the figure - in the corners of the power spectrum. Why are these not considered

'A' is used to denote Angstrom - please use the correct Angstrom symbol.

The phase of the SSB reconstruction seems strange to me. The Pb columns should have the highest phase shifts, it is not cleat that thy do. In fact in some places the MA+ column seem to have the highest phase shift. Please comment on this.

Supplementary fig 9. d): Please explain the yellow markers in the caption. f) should be labled orthorhombic not orthogonal.

Supplementary figure 10.: What Fourier filter is used to create the images in b and d. The use of Fourier filters, in which only specific, user defined frequencies from an original image are passed to the filtered image can create significant artefacts in images.

Addition information is required to explain the classification of regions as tetragonal and orthorhombic. b and c show no immediately discernible difference. Do the arrows highlight positional variation? If so this needs to be explained in detail and quantified. The coloured dots on the image b and d need to be explained in the caption.

Supplementary figure 11, caption: "The resolution achieved by the ePIE phase image is significantly higher than that of ADF images."

How much higher? Please quantify.

Again Supplementary figure 11 k) shows phase wrapping on numerous atomic columns. There also seems to be strange phase contrast with heavy atomic sites sometimes appearing brighter (larger phase shift) and sometimes not - though this may be due to phase wrapping. This needs to be resolved in the presentation of the data or explained in the text.

Line 162: "Ultra-low-dose 4D-STEM imaging techniques provide atomic-resolution HAADF, LAADF, iCOM, ePIE, and SSB images (Supplementary Fig. 11-12). Our LAADF and phase images provide enhanced visualization ... "

These comments are misleading. The HAADF is not from the 4DSTEM data set and the LAADF could be collected using a standard monolithic detector. If your claim is that the alignment between scans is only possible due to the higher signal to noise of the ptychographic reconstructions which then enable the summation of the virtual-LAADF and HAADF then this needs to be made explicit.

Supplementary Figure 13: The measurements on d) and e) are unclear and the labelling is too small.
The C-N from the MA is shown at a single atomic column position. At all other positions in the reconstructed image they are

not resolvable. This makes me sceptical about the claim the the C-N atoms within the MA molecule can be resolved. Why is this only shown at one atomic column position? Additional examples showing resolution of the C-N atoms need to be presented or a reason why only one example was found needs to be discussed in the text.

Line 181: "By the 10th to 12th scans, Region I transitions from its original perovskite structure into lead iodide,"

This is not made clear in the figure - the reader needs to be informed what the structure of PdI2 is and the change needs to be highlighted on the figure with for example an atomic model overlay, or measurements of bond lengths.

Line 187: "virtual ADF"

Previously this has been referred to as LAADF - please remain consistent throughout the document.

Line 187: "The averaged images and intensity profiles(Fig. 3d-3f) show Region I has iodine defects and a lead-terminated edge, while Region II has minimal defects and terminates with iodine and MA. Models illustrating these observations are shown in Fig. 3g and 3h."

I don't agree with this interpretation. The ePIE and SSB averaged reconstructions, shown in Figure 3d clearly show iodine termination, all be it with slightly lower intensity than in Fig 3e. it is possible that this is due to a single iodine atom terminating, thought this would need to be backed up by simulation. Due to the phase wrapping problem (causing dips in the centre of the Pb columns) it is difficult to quantitatively compare these results. I don't see any evidence of MA termination in the ePIE averaged reconstruction of fig 3e). I cannot agree that the imaged presented in Fig 3d and e conclusively demonstrate the atomic models shown in g and h.

Line 192: "The images show a progressive reduction in MA+ at the edge, commencing from the initial scan, underscoring the susceptibility of MA..."

I think this is incorrect. I can see no evidence of MA termination if Fig 3 i)-1. The edge seems to be Iodine terminated.

Line 200: "By the 7th scan, PbI2 began forming."

Where/what is the evidence of this? Which figure is it shown in? Please provide additional information

Line 201: "Supplementary Fig. 18), we observed that most edges are terminated with I– and MA+ 202 , which is consistent with the DFT calculations "

Again I don't agree with this interpretation. I can see very little evidence in Fig 18 of MA termination. It appears almost exclusively Iodine terminated.

Line 205: "These observations underscore the rapid collapse of the crystal structure induced by defects, emphasizing the substantial impact of edge structures on material stability."

What is the structure of these edge defects which induce rapid collapse? Please add additional information.

Line 217: "However we have also identified additional forms of defects, specifically, Pb defects and I defects, which are clearly visible in the phase image..."

The data presented is not sufficient to support this conclusion. Without comparison with simulated phase or projected potential images it is not possible to evaluate whether the experimental data supports the proposed atomic models.

Line 228:"...but it forms away from the defects..."

Supplementary figure 19 does not conclusively show this. Although the yellow arrows do in some instances point to regions of the sample in which the MA column position is similar intensity to the Pb-I columns it is not clear that this is PbI2. At the very least the reader needs to be informed on the structure of PbI2 and what features in the images confirm that this has been identified.

Line 235: "Our observations show that I defects are unstable centers, while Pb defects are stable ones."

I do not believe that the evidence supports this claim. The study of edge termination only showed one example in which it appeared as if the Pd-I terminated edge was destroyed faster that the Iodine terminated (figure 3). For bulk defect one example (figure 5) is shown which appears to be both Pb and I deficient and then grows more or less isotopically with increased electron dose. At no point in the paper is there a comparison between the stability of Iodine and Lead defect sites.

Supplementary figure 19
Typographic errors in the caption PhI2 should be PbI2.

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)
The authors have made substantial revisions in this version and added significantly more data in the Supplementary Information to support the claims and illustrate the technologies in more detail. The image quality as well as the presentation quality have also been improved. The authors have successfully addressed my questions in my previous review to my satisfaction. I also noticed that the contrast reversal issues of heavy atomic columns in the previous version have been corrected using the phase offset method, which makes the images more legible.

In general, I am satisfied with the revision. However, I request the following minor revisions before the manuscript proceeds to publication:

1. Abstract: The tone of the abstract could be elevated to match the achievement of this work that qualifies its publication in Nature. Important achievements such as the first-time observations of atomic-resolution edge structures and dynamics using the lowest dose could be emphasized, echoing the relevant statement in the main text and Conclusion.

2. Section 2.3 – Interior region defects: This section needs to be rephrased to make the main text more logical and self-contained, though readers could get the message the authors are trying to convey by referring to the relevant sections in the Supplementary Information. After mentioning “Defects within the interior of the crystal can also strongly affect stability,“ it is better for the authors to first mention that they found two types of defects associated with different net ionic charges that show different stabilities, and then elaborate on the two types of defects.

3. Supplementary Information, Page 32, last paragraph: “the MA columns at the edge appear to be slightly closer to the I columns in the Pb-I columns in the next layer inside the material” – What does this exactly mean? It is not clear, though the readers might have the right guess. I recommend adding two pairs of vertical lines in the line profile to illustrate the distance difference, instead of just saying “appear to be.”
4. Line 94, It is not clear what the authors are trying to say here; “With the small probe step size we used to keep the probe moving as much as possible”. Please make some explanation or rephrase it.

5. There are some errors that need corrections:
a) Fig.2 Please also add the annotation to the color circles/dots as in Fig.1.
b) Fig. 3 (f) should be (e); Suggest to add scale bar in every experimental image here.
c) SFig. 46 (d), the dose should be 12.6×3;
d) The last sentence of SFig. 47 annotation needs to be corrected to fix grammatical errors;

Referee #2

(Remarks to the Author)
In this work the authors present a study of the structure of defects and induced electron beam damage in methylammonium lead iodide (MAPbI), an organic-inorganic perovskite with promising semiconducting properties for potential applications including in next generation solar-cells. The authors show that by using electron-ptychography (which is known for its high imaging efficiency) the atomic structure at edges and defect sites can be determined and that the presence of defects increases the rate of crystal breakdown under illumination with the electron beam.

The images presented represent the lowest electron dose ptychographic atomic resolution imaging to date (to the best of my knowledge) and the technique has been applied to a technologically important class of materials which are challenging to image at these resolutions. As the authors acknowledge bulk defects and grain boundaries of MAPbI have previously been imaged at atomic resolution using LAADF STEM at doses of 60-100 e/A^2 [Rothmann, M. U. et al. Science 370, eabb5940 (2020).] Other beam sensitive materials have previously been studied using broad beam TEM illumination at doses of a few 10's A/^2 [Zhang, D. et al. Science 359, 675–679 (2018).] equivalent to the doses used in this study.

The main focus of the paper is to identify that the electron beam damages the samples significantly quicker at iodine deficient edge locations and at bulk defect sites. This is a generally unsurprising result. The authors do not provide a detailed statistical analysis of edge structure nor provide a detailed study of various bulk defect structures which would be of general interest to the halide-perovskite community and inform on future material understanding and fabrication.

The data presented is of a high quality with and analysis is generally sound. I have some concerns over the attempts at phase quantification (this has been notoriously challenging for the electron microscopy community for many years) and the subsequent assignment of "net ionic charge" states. These concerns are detailed below.

In my opinion this study does not reach the high bar for general interest which is required of a publication in Nature and I cannot recommend for publication. I recommend resubmission to a journal with more focused readership after addressing the specific comments below.

specific comments:

Line 56 - "less than one thousandth the dose typically used for stable samples.[14, 17]" This statement is misleading. Reference 17 is a demonstration of atomic resolution TEM imaging of MOF samples at cumulative doses of 10-20 e/A^2, perhaps double the approximately 6 e/A^2 dose budget for MAPbI stated in the text.

Line 88 - "In fact using multiple scans the material withstands between five to ten times the dose than in TEM (see SI)" The authors show that, using broad beam electron diffraction the MAPbI samples damage at electron doses of a few 10s e/A^2 (supplementary figure 6). The analysis of this broad beam diffraction data is fundamentally an average of the whole illuminated area (a large crystal) which likely includes a large number of defects and unstable edges. In fact, the major focus of the manuscript is that defects are susceptible to damage at these low electron doses. In my opinion the data presented in the supplementary information does not provide convincing and conclusive evidence that a low dose high resolution TEM study would be unable to image defects and edges of this material.

Line 102: "In contrast ptychography makes use of the direct beam, and provides much greater overall dose efficiency." - "much greater" is subjective, please drop the 'much' or quantify.

Figure 2 caption: "Power spectra (cropped) of a and e" - Typographical error, this should read "a and d".

Figure 2e) the identification of the 1.1 Angstrom reflection in the power spectrum is not convincing, this appears to be just noise in the power spectrum not a clear periodicity.

line 108 "The ptychography however is more efficient, and its significantly clearer images allow us to reveal the locations of all the atomic columns, including the MA+ molecules, as is also seen in simulations (SFigs. 25–28)." I am not fully convinced that the MA site is visible in figure 2(f). Furthermore in the simulations in figures S26 and S27, the MA molecules are not visible at doses equivalent to the experimental data shown in figure 2f (36e/A^2).

Figure 3 - caption. Please add a description of the line scan in panel (b) to the caption.

Figure 3 - caption "(d) and (f)..." Typographical error, this should read "(d) and (e)" there is no (f) in the figure.

Line 141 - "Region 1 exhibits both loss of material and formation of Pb crystallite (red box, Fig. 3e)." For the general reader please briefly explain how you come to the conclusion that a Pb crystallite has been formed (or refer the reader to supplementary information).

Line 162 - "Defects within the interior of the crystal can also strongly affect stability. Rather than transitioning to orthorhombic after 32 e–/°A2, this region damages much more rapidly and severely, appearing to lose significant material in an area around five times the initial defect size by 128 e–/°A2." I believe you are referring to the images in Figure 4 panels a) - c) here please state this as it is not clear to the reader. Panel 4 c) is labelled 192.6e/A^2 where as the text refers to 128 e/A^2. Is there a reason that the higher dose image is shown in the main text?

Figure 4 - The proposed structures of the 'stable' defects are shown. What is the proposed structure of the 'unstable' defect as imaged in panel a)?

Line 178 - "The destabilising defect in Fig. 4a, contains a high proportion of I– vacancies producing an estimated net ionic charge of +2.5." I cannot find the proposed structure of the 'destabilising' defect in the main text or in the SI. As the relatively high 'net ionic charge' of this defect with respect to the two other identified defects is one of the conclusions of the work it is necessary to show the analysis leading to this conclusion. This can be added to the SI.

Supplementary Information - "Estimating of defect charge states"
I feel that there is insufficient information provided to be fully convinced by this analysis. Specifically:
* Over what distance about each atomic centre is the phase summed?
* What is the variation of phase about nominally identical columns in the pristine region of the material? Can this be used to assign an uncertainty to the measurement?
* What is the thickness limit over which the phase of, particularly the PbI column can be considered to be linear with thickness?
* The nanosheets have previously been determined to be approximately 3 unit cells thick. Does the quantification of phase here always give integer number of atoms? If not why not?

Version 2:

Reviewer comments:

Referee #1

(Remarks to the Author)
This time, I have only a few technical issues that require the authors' attention. However, I urge the authors to take the technical quality of the paper more seriously and conduct a thorough review to eliminate similar errors throughout the manuscript. I will recommend acceptance once these issues are properly addressed.
1. Dose notation consistency:
The dose notation must be consistent throughout the paper. Please correct the dose information in the following figures, and carefully review all other figures that contain dose values:
o Supplementary Figures 45c, 45f; 46c, 46d, 46e; and 50d.
(Note: This issue was already pointed out in the previous review—specifically, Supplementary Figure 46d in the earlier version.)
2. Figure caption errors:
There are still several mistakes in the figure captions. For example:
o Figure 2: The phrase "... of a and e" should be corrected to "... of a and d"; the final reference to "f" should be "d".
3. Supplementary Document, Page 34 (last paragraph):
o The sentence: "...appear to be slightly closer to the I columns in the Pb-I columns in the next layer inside the material" is unclear and should be revised for clarity.
o Additionally, the two added pairs of lines in Supplementary Figure 39 should be explicitly mentioned and explained in the text.

Referee #2

(Remarks to the Author)
In this second revision the authors have again made significant improvement to the manuscript adding substantially to the SI. All comments from my previous reviews have now been addressed either through changes and additions to the manuscript and SI or through robust rebuttal.

This work represents an impressive experimental acheivement, data analysis is comprehensive and conclusions well supported with extensive simulation. The manuscript is very well written and will be of interest to scientists studying metal-halide pervoskites and to the wider electron imaging community.

I found one additional typographical error in the supplimentary information, page 46:
" Therefore the phase of the Pb-I columns also begins to decrease above about 4.7 nm, but this does not matter **for the purposes of our present purposes** as our sample does not reach this thickness."

Referee Response

We thank the reviewers for their valuable constructive criticism, and recognition of the significance of our accomplishments despite the flaws in the initial submission. We have rewritten the manuscript in response, improving the presentation of both the text and figures. We include additional data and a greatly expanded Supplementary Information (SI) which now includes discussion on the details of topics which were previously too tersely addressed.

Importantly, we have improved the display of the images. Many images in the previous submission were displayed improperly, preventing important details from being visible. We also removed the undesirable contrast reversals on the Pb-I columns that complicated the interpretation of the ptychography using our recently published phase offset method.

We respond to the referee comments point by point below.

Referee 1

> *"Leveraging the high-speed, event-driven Timepix3 hybrid pixelated direct electron detector, the authors reveal the atomically resolved structure of one of the most beam-sensitive materials, $MAPbI_3$ hybrid halide perovskite, using the 4D-STEM ptychography technique under ultra-low dose conditions of just a few tens of electrons per square angstrom. Notably, the authors integrate the dose fractionation technique, widely used in CryoEM and HR-TEM, into 4D-STEM ptychography, enabling them to elucidate the dynamics of the more vulnerable edge and defect structures under electron illumination for the first time. The resolved termination configurations of the film edge and defect edge allow the authors to correlate these configurations with the stability of nanosheet and defect edges under electron beam illumination. This, in turn, leads to informed recommendations for optimizing the film synthesis process to enhance performance.*
>
> *The results demonstrated, for the first time, the exceptional capabilities of 4D-STEM ptychography in delivering atomic-resolution imaging of ultra-beam-sensitive materials, such as hybrid halide perovskites, a long-anticipated achievement (Ref. 1). Even*

more impressively, its extraordinarily low-dose handling capabilities enable the dynamic study of such beam-sensitive materials. Considering the advantages of 4D-STEM ptychography over other commonly used imaging techniques (HR-TEM, ADF-STEM, iDPC, etc.), including easier image interpretation, better contrast for both heavy and light atoms, greater flexibility in beam focus, and improved tolerance to sample thickness, these results are expected to catalyze research interest within the community.

The paper contains impressive data; however, significant content reorganization/ rewriting, and improvements in data presentation quality are necessary before it can be recommended for publication. The authors are requested to address the following questions and comments."

We thank the reviewer for their very positive remarks as well as the constructive criticism. We have performed a major revision addressing the reviewer's points, which we respond to in details below.

R1#01

"The abstract concisely summarizes the key achievements of this paper. However, there is insufficient experimental data to support the claim of "acquisition of atomic-resolution edge and defect structures of perovskite at a dose of 10 $e^-/Å^2$." The single-frame image with a dose of 13 $e^-/Å^2$ (SFig. 17) is not clear enough to reveal the atomic structure at the edge. While Fig. 3, which is a sum of three frames, does reveal the edge structure, but without dose information. In the same figure, the averaged single frame data shows significantly higher S/N ratio, but this should be mentioned in the claim, as it is an averaged representation. The clear atomic defect structures in the interior region (Fig. 4d, g) are obtained at doses of 127 and 109 $e^-/Å^2$, respectively. Therefore, the authors should either provide additional robust experimental data to support this claim or revise the claim to reflect the presented experimental data. Including a table in the Supplementary Information that summarizes the imaging conditions (including dose) for all the images is recommended."

Thank you for pointing this out. Many of the images were improperly displayed in the previous manuscript documents, with brightness and contrast levels which hid much of the

details. They should indeed have also had the dose clearly indicated. We have rectified these issues in the revision.

It should now be apparent that atomic resolution is indeed obtained at approximately 11 $e^-/Å^2$, although of course the finer details of the edge are much clearer as the dose fractionated data are combined at higher effective dose.

Although it clearly depends on making atoms or atomic columns visible and discernable atomic resolution is not a strictly defined term in terms of clarity. Conventional atomic resolution ADF images frequently do not reveal all the atoms. A typical example is atomic resolution ADF imaging not resolving light elements when neighbouring heavier columns. Atoms are clearly visible in our images of the edge from the individual low dose scans as shown now in SFig. 42. These images also show how atoms are resolved further inside the crystal, as illustrated further in SFigs. 19, 29 and 30. Therefore it seems correct to identify the images from the individual scans, which are generally taken around approximately 11 $e^-/Å^2$, as atomic resolution. Of course, we recognize that the finer details of both the edge and defect structures are clearer at higher doses.

Therefore, we now

- State in the abstract that we obtain atomic resolution at electron doses as low as tens of $e^-/Å^2$, which is in line with the accumulated doses used with the dose fractionated data used in Fig. 3.
- Explicitly discuss the use of dose fractionation and indicate the accumulated dose in all figures.
- Include a table in the Supplementary Information (SI) summarising the imaging conditions including dose as requested

R1#02

> *"In the conclusion section, two of the claims—"imaging can be achieved with approximately 10 $e^-/Å^2$, a temporal resolution of 0.52 s, and an Abbe spatial resolution of 1.2 Å"—are questionable, as there is no convincing evidence that the 10 $e^-/Å^2$ dose and 1.2 Å resolution occur simultaneously. The 1.1 Å resolution of the bulk structure*

(Fig. 2) is achieved at a dose of 32 $e^-/Å^2$, and the 1.25 Å resolution at a dose of 10 $e^-/Å^2$ is based on simulated dataset (SFig. 7a, d). There is no specified resolution for the single-frame experimental data at a 10 $e^-/Å^2$ dose (SFig. 11 i, j), possibly due to the high noise level in the FFT. Additionally, the claim of "a temporal resolution of 0.52 s" only appears in the conclusion and should have been mentioned earlier in the imaging section. A rewrite of this section is necessary."

We follow on from our response to R1#01 by noting that a spatial resolution of 1.5 Å can be observed in the present SFig. 30 at 10.6 $e^-/Å^2$, which corresponds to a revision of Fig. S7 in the original submission. In the initial figure, these experimental data were unfortunately incorrectly labelled as simulated data. This mistake has been rectified in the revised manuscript. Please note that a resolution of 1.5 Å is sufficient to resolve the spacings in $MAPbI_3$.

We do however agree that the language used previously needed improvement, and have rewritten the conclusion, and other references to resolution, to ensure they are correct and clear. We have also added a section in the SI called "Experimental comparison of different imaging modalities" in which we discuss the experimental results from different methods and show power spectra from experimental data using single scans and multiple dose fractionated scans in SFigs. 29 and 30.

As the time between scans, which was referred to as temporal resolution previously, does not seem particularly relevant here we have removed it from the conclusion.

R1#03

"The authors provide good references to previously published work. However, to highlight the significance of this paper, it is recommended that the authors comment on a closely relevant paper by M. U. Rothmann et al. (Ref. 2) to demonstrate the advantages and uniqueness of the 4D-STEM ptychography technique. The authors may also consider replicating the experiment using an annular dark field detector and comparing the results in the supplementary information. In that reference, Rothmann et al. studied a similar hybrid perovskite material ($FAPbI_3$) using low-angle annular dark field (LAADF) STEM technology, employing a standard STEM annular dark field detector

with image processing filters under an electron dose as low as 53 $e^-/Å^2$. Although they claimed to have imaged $MAPbI_3$, no relevant images were provided. This naturally raises the question of whether STEM ptychography is necessary for this type of study. Addressing this issue is crucial, as it impacts the perceived importance of your paper."

Thank you for the suggestion. We agree it is beneficial to discuss Rothmann et al.'s study and LAADF, and have done so in our revision.

As we now note in our revision, the $FAPbI_3$ specimen used by Rothmann et al. was relatively thick. The approximately 30 nm thickness made it possible for them to image the thick columns of FA cation molecules using LAADF, which would otherwise not be sufficiently sensitive to discern the light molecules at the very low doses required to avoid damage. However, relying on thickness to detect columns means losing sensitivity to finer structural details. This includes losing sensitivity to few-atom defects such as vacancies, both within the crystal and at the terminating layer of edges. As these are likely to catalyse damage or potentially protect against it, we require a thin sample to allow us to detect the finer details of the structure. In SFig. 2 we demonstrate the lack of sensitivity to few-atom vacancies in 30 nm thick $MAPbI_3$ and how much stronger a change in contrast is produced by few-atom vacancies in 3.6 nm thick ultrathin $MAPbI_3$. This is why we used ultrathin $MAPbI_3$ in our experiments, in which the removal of a few, or even individual atoms, causes a proportionally far greater change in contrast that can be detected at the extremely low doses required to both avoid damage in the initial images and capture the dynamics as it sets in.

However, the strength of the LAADF signal decreases as the sample is made thinner as there is much less scattering to use for the Z-contrast signal. Because of their high atomic number, the PbI columns still stand out in the LAADF images of ultrathin $MAPbI_3$, but overall the images become much more noisy as seen in Fig. 2. The fine details become difficult if not impossible to discern reliably. The MA atoms are invisible, and as seen in Fig. 3, columns containing vacancies have very low contrast indeed. It seems clearly desirable to add the substantially stronger signal of the ptychography alongside that of the LAADF.

In addition to modifying the main text to discuss these points, we have added a section discussing this in detail in the SI called "The need for ultrathin $MAPbI_3$ nanosheets and ptychography". This includes discussion of the simulations showing how LAADF imaging of

a 30 nm thick $MAPbI_3$ sample is not sensitive enough to show vacancies consisting of several atoms. We have also added significant additional discussion and figures comparing LAADF and ptychography in the SI including in the new sections entitled "Simulated imaging of the edge structure of $MAPbI_3$" and "Experimental comparison of different imaging modalities".

R1#04

> *"Proper image annotation is missing in this paper. None of the color-coded atomic models are labeled, making it difficult for readers unfamiliar with the structure to interpret the images."*

We have fixed this now.

R1#05

> *"For the film edge termination, the authors should include a Zoom-in inset with arrows indicating Pd/I column and MA columns."*

We agree the presentation of Fig. 3 should have been clearer regarding the identification of the atomic columns. We believe this is best addressed in our new figure with the model shown alongside the ptychography simulations in addition to the updated overlay on the experimental image.

R1#06

> *"In Fig. 3C, there is a claim that $MAPbI_3$ transforms into PbI_2, but no obvious supporting evidence is provided. An atomic model showing the lattice similarity along a specific alignment of the two phases (e.g., (1-11) PbI_2 / (001) $MAPbI_3$) should be included to help readers easily verify that the PbI_2 phase has indeed formed as claimed. Additionally, at least one arrow should be used to point to the location where this transformation occurs."*

We agree the presentation was lacking. Furthermore, after reexamining the data we believe the material in this particular region is actually more I deficient than initially thought and forms Pb metal rather than PbI_2. This conclusion is based primarily on the lattice spacings, as we now state in the revision and support with additional figures (SFigs. 40, 41, 44)

and discussion in the new SI section entitled "Termination of the edge of MAPbI3 and its evolution under electron irradiation". Additional images of the region and its evolution are displayed in SFigs. 40 with power spectra. Both the contrast and lattice spacings of this region match those of Pb metal after damaging. Models comparing the lattices of $MAPbI_3$, PbI_2 and Pb metal are shown in SFig. 7 alongside simulated diffraction patterns. An annotation has also been added to Fig. 3 to indicate the location of the transformation.

Furthermore, additional discussion of the observed transformations has been added to the SI, including in the new sections "Damage rate under conventional plane wave TEM illumination", "Evolution of pristine $MAPbI_3$ under electron irradiation", and "Effect of internal defects on stability".

R1#07

> *"In SFig. 19, the arrows indicating the PbI_2 phase formation should be placed more accurately; some are off by about one unit cell, which could confuse readers. Additionally, there is a confusing typo in the caption; "PhI_2" should be corrected to "PbI_2." Throughout the paper, there are several misspellings and grammatical errors that should have been corrected before submission."*

We agree and have fixed these presentation issues, including the misspelling and grammatical errors.

R1#08

> *"In Lines 83-84, the authors mention that a focused probe was chosen due to the distinct nature of the sample. Since this is a technology-focused paper, a more detailed explanation is warranted. Are there additional reasons for this choice, such as the possibility that certain algorithms (e.g., SSB, Ref. 3?) may not perform well with defocused probes? Typically, it is believed that a defocused probe is more dose-efficient (Ref. 1), and one of the advantages of STEM ptychography over iDPC is that it does not necessarily require a focused probe. Additionally, creating a focused probe might incur a higher dose cost."*

We have revised this section, and now point to Rothmann et al.'s study supporting the

higher dose tolerance of halide perovskites to a rapidly scanned focused probe than plane wave TEM, as well as the model of Jannis et al. which can explain this tolerance. The model suggests that materials such as $MAPbI_3$ will tolerate multiple very brief exposures to the beam better than a continuous exposure as in TEM. A defocused probe configuration will, like TEM, typically use a significantly longer and more continuous exposure compared to a focused very rapidly scanned probe. To discuss this in further detail we also added a section entitled "Discussion of $MAPbI_3$ dose tolerance in STEM and TEM"

Ref. 1 of the review does not appear to address defocus in the context of ptychography, however it is true that iterative ptychography tends to converge more easily when individual diffraction patterns have more electron hits in comparison to sparse data. This means that for a given dose, convergence of iterative ptychography can be easier with a defocused probe in combination with a significantly longer dwell time, but this is not the same as dose efficiency. There is to our knowledge no convincing demonstration that a defocused probe is inherently more dose efficient, nor do we see why this would be the case physically. Practically, the slow speed of 4D STEM cameras has led to defocus being used as a route to achieve low dose imaging, however this precludes the use of signals that require a focused probe, including the LAADF that has so far been the most successful signal for imaging halide perovskites. Maintaining a LAADF signal does indeed seem beneficial, both in terms of reliability and interpretability. Practically, it also allows one to see what one is doing while collecting data, and an SSB ptychography image is far faster to obtain than an iterative ptychography result.

Regarding the dose cost of creating a focused probe, we agree poor experimental design could indeed incur a higher dose cost if time is spent focusing the probe in an inappropriate way, but our experimental procedure precludes this. With our procedure a fine STEM probe actually allows for more accurate focusing than TEM because it can be performed nearer to the region of interest than with a broad TEM beam . Furthermore, a focused probe facilitates accurate navigation of the sample, and the need for precise focusing is reduced by the large depth of field provided by our modest convergence angle.

We now discuss our procedure for setting up data collection with minimal exposure to the area of interest in our revised SI in the section entitled "Data acquisition protocol for dose

minimisation".

R1#9

> *"The authors could emphasize the low-dose handling advantage of STEM over TEM by referencing SFig. 3 and quoting the TEM results from Ref. 1 at room temperature and Ref. 4 under cryo conditions (-175°C). At room temperature, both SFig. 3 and Ref. 1 show that $MAPbI_3$ begins to degrade at a dose as low as 6 $e^-/Å^2$, whereas under cryo conditions, the critical dose for $MAPbI_3$ is 12 $e^-/Å^2$. Furthermore, if the authors could provide some insight into why this material can tolerate a higher dose in STEM mode compared to TEM mode (even if this is beyond the immediate scope of the paper), it would significantly enhance the value of the paper. Many people believe the opposite is true."*

In our revision we reference these references and the figure, as well as additional figures we have added to the SI alongside additional discussion. We also provide the suggested insight into the higher dose tolerance of using multiple very rapid scans with a focused probe in STEM compared to the broad continuous illumination TEM using the discussion and models published by Velazco et al. and Jannis et al. (doi.org/10.1016/j.ultramic.2021.113398 and doi.org/10.1016/j.ultramic.2022.113568) in a section entitled "Discussion of $MAPbI_3$ dose tolerance in STEM and TEM".

R1#10

> *"As dose is a critical parameter in this study, the authors should describe the specimen finding and beam focusing procedures in detail. Specifically, how much dose does it take to find the sample and focus the beam, and is there any protocol to minimize the dose of pre-imaging? In HR-TEM imaging, pre-HR imaging handling is also crucial, as the sample might be damaged even before imaging begins (Ref. 5). Therefore, providing this information is important for readers."*

Yes, we followed specific procedures to navigate and setup imaging. We agree this should be described, as we do now in a section of the SI entitled "Data acquisition protocol for dose minimisation".

R1#11

> *The samples studied in this paper are very thin, only a few nanometers. Conducting an analysis similar to that in Fig. 2 on samples with a thickness of tens of nanometers (e.g., 20 to 50 nm) could demonstrate the robustness of this technique, broaden its applicability, and attract the interest of users who work with thicker samples.*

Indeed, we intentionally worked with ultrathin material to enable defects consisting of only a few atoms to be detected. We have no doubt that we could image a thicker region in a similar manor as Fig. 2. A thicker material would after all have a stronger potential and therefore create stronger looking images. We have demonstrated methods for handling thick materials in various publications including 10.1063/5.0101895 and 10.1016/j.ultramic.2024.113922 which we already cite. However, it would still be more challenging to detect the few-atom vacancies in a thicker material. We therefore choose to restrict our experiments to utlrathin material in which we can detect few-atom vacancies.

R1#12

> *"The ultra-fast event-driven hybrid detector is a key enabler for this work. The authors are encouraged to briefly explain why they chose this type of Timepix camera over other commonly used cameras, such as the Medipix camera. This information would assist users who are less familiar with these technologies in selecting the appropriate camera for their applications. This discussion could be included in the Supplementary Information. I am not familiar with the dose calibration using the Timepix camera, so I cannot comment on this aspect."*

The Timepix3 enables the exceptional 4D-STEM speed we require here as we now note in the updated section 2.1 of the main text. We have also added a section in the SI entitled "Event driven 4D-STEM with Timepix3" to describe this in more detail and how this compares to other 4D STEM cameras, including the Medipix3 and the newer Dectris Arina.

Referee 2

> *"In this work Yuan and colleagues present a low electron dose study of the edge structure and defects in lead halide perovskite materials. They employ electron ptychography to form atomic resolution images at low electron dose which with resolution greater than the corresponding annular dark field images and show a breakdown of crystallinity via mass loss with increased cumulative dose.*
>
> *The approach builds on established work in the field for low electron dose ptychographic imaging [e.g. Scientific Reports 9,3919 (2019), Appl. Phys. Lett. 116, 124101 (2020)] and does represent, to the best of my knowledge, the lowest dose example of atomic resolution ptychography. However these materials have previously been successfully imaged at atomic resolution using conventional aberration corrected TEM imaging by Rothmann et al. [Science 370,eabb5940(2020)] all be it at slightly higher electron doses.*
>
> *In this work Yuan and colleagues successfully image the atomic edge structure of small, thin MAPbI flakes, to the best of my knowledge the first time this has been achieved.*
>
> *Though this represents a significant achievement I cannot recommend publication of this manuscript in Nature for the following four primary reasons:"*

We thank the reviewer for recognizing this as a significant achievement and for their constructive criticism. We have performed a major revision of our manuscript in response and we address the individual issues point by point below.

R2#01

> *"The quality of the ptychographic reconstructions is significantly poorer than the state of the art in the field. This is to be expected for very low dose experiments but there are clear and obvious deficiencies in the ptychographic reconstructions probably due to phase wrapping which are not addressed at all."*

We agree that the presentation of the images was regretfully poor. This is due to two main factors:

1. Improper brightness and contrast were used for the displayed images.

2. The contrast reversals were not addressed.

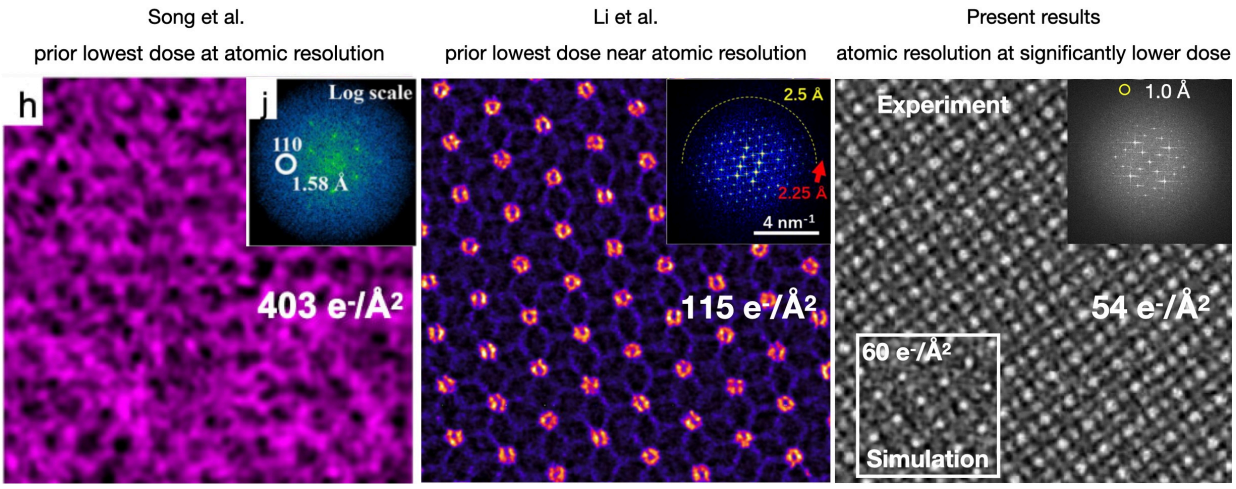
Both have been addressed now.

The deficiencies the reviewer refers to in the ptychographic images as "probably due to phase wrapping" can only be assumed to be the dips that occur on the heavy Pb-I columns. These are what are known in the electron ptychography community as contrast reversals. Contrast reversals have been observed since at least 2017 (10.1016/j.ultramic.2017.02.006), and have since been the subject of multiple papers (10.1016/j.ultramic.2023.113879, 10.1093/micmic/ozac022, 10.1016/j.ultramic.2023.113879, 10.1016/j.ultramic.2024.113922), and are in fact expected. Contrast reversals occur in simulations of both iterative and direct ptychography, including in noise free data, and are not specific to our experimental data. It is also worth noting that they are not specific to ptychography, as they can also appear in iCoM images as shown by Clark et al. (10.1093/micmic/ozac022). The published results showing contrast reversals also include simulations of ultrathin $MAPbI_3$ (doi.org/10.1016/j.ultramic.2024.113922).

While the contrast reversals are expected we agree we should have addressed them and indeed counteracted them as we do now using our newly developed phase offset method of contrast reversal correction. We have also added a section in the SI discussing the contrast reversals and the use of the phase offset method to counteract them entitled "Contrast reversals and their counteraction".

Therefore, rather than being poorer than the state of the art, our results in fact extend the state of the art, representing the the best electron microscopy can provide for ultralow dose ptychography and halide perovskites generally.

To illustrate this further, we compare an example from our present results to the lowest dose previously published atomic resolution and near-atomic resolution ptychography results in RFig. 1. Our 54 $e^-/Å^2$ experimental ptychography results are obtained at substantially lower dose than the prior lowest dose atomic resolution work published by Song et al. (https://doi.org/10.1038/s41598-019-40413-z) at 403 $e^-/Å^2$, but provide substantially clearer images. The image we display here is also providing clear atomic resolution at around half the dose of the lowest previously published near-atomic resolution work of



RFig. 1. Comparison of the previous lowest dose atomic and near-atomic resolution ptychography results to results from the present study, which are clearer despite using a significantly lower dose. Note that the bright features in Li et al.'s image are not individual atomic columns, but rings of columns.

Li et al. (https://doi.org/10.1038/s41467-025-56215-z) at 115 $e^-/Å^2$. Note that the bright features in the image from Li et al. are rings of atoms, not individual atomic columns. We overlay a simulated three unit cell thick $MAPbI_3$ ptychography image over our experimental image. The simulation uses 60 $e^-/Å^2$ a similar dose to our experimental data and the same voltage and convergence angle. We do not expect a perfect match, but the similarity of the simulation to the experiment seems convincing that the experimental results match expectation for the dose.

In our work we also use results from lower doses than 54 $e^-/Å^2$ as we wish to study the changes that occur at these lower doses, and these are naturally generally a little less clear. However, especially with the contrast reversals counteracted, we do not believe they can be said to be of lower quality than the state of the art because of this.

R2#02

> *"The authors primary conclusion is that edge termination and defects have a major impact on the material stability. The experimental evidence presented for different edge terminations is not conclusive and the authors do not determine the structure of specific classes of edge defect and correlate these with stability. The data presented is from a single edge site and a single bulk defect. In order to come to robust conclusions*

regarding material structure would require numerous examples of each to be presented."

We strongly agree that a correlation of structure and material stability should have been presented and therefore do so now in the revised manuscript. This allows a much more robust and informative discussion of the mechanisms at play.

R2#03

There is very little quantitative analysis of bond lengths and no attempt to compare the reconstructed phase images with simulation to validate conclusions regarding atomic structure.

This was partially another very regrettable presentation issue as the simulation in the intial submission was mislabeled. However, we believe the simulations should have been more prominent and numerous.

We have now placed simulations of the edge prominently in the main text as part of Fig. 3, and included many additional simulations in SFigs. 13, 14, 15, 18, 16, 25, 26, 27, 33, 25. They should now be correctly labelled as simulations.

We also now provide annalysis of the bond lengths / lattice spacings, which we certainly agree is essential. In depth discussion is presented in the SI and referred to in the main text.

R2#04

The manuscript is peppered with unsubstantiated claims, omissions and inaccurate interpretation of data (detailed below) and the authors draw conclusions which are almost wholly unsupported by the presented experimental data (detailed below).

We agree that the presentation was poorly lacking and have corrected this in our major revision. We hope that the reviewer will agree that these changes address these issues.

We respond to the individual detailed points of the reviewer below.

Detailed points of Referee 2

R2#05

> *"Strictly speaking radiation dose is a measure of energy deposited and has units of Grays (Gy). The correct term for 'number of incident particles' is particle fluence which has units of number of particles per unit area. However the two terms (dose and fluence) are used interchangeably in the literature."*

Indeed, the literature uses dose and fluence interchangeably, with dose being predominant in this particular field in our view. Therefore we follow the example of Rothmann et al.'s study representing the best previous imaging achieved in lead halide perovskites, and use the term dose. This enables the use of the term dose fractionation, which we believe is useful in this context. We have added '(fluence)' after the first use of dose in the main text.

R2#06

> *The references are oddly numbered, alphabetically which is not standard for the journal and makes following references more challenging for the reviewer. Please reformat in the journal standard style.*

We have reordered the references.

R2#07

> *Line 83: "We opted for a scanned focused electron beam due to the distinctive nature of the observed sample, $MAPbI_3$*
>
> *What does ' distinctive nature of the observed sample' mean? This isn't a justification for the chosen optical setup."*

This referred to the material damaging less under a rapidly scanned focused probe in comparison to wide area illumination plane wave TEM.

We have revised this section, and now avoid this abmiguity.

R2#08

> *"Line 94: "...and the thickness of similar nanosheets are in the range of 1-3 nm using electron energy loss spectroscopy"*
>
> *Poor English language. Please provide more information in the SI regarding how the thickness was calculated from the raw spectroscopic data. Specifically what technique was used and what value for inelastic mean free path was choosen and with what justification?"*

The minor grammatical error has been removed in the revision. We also now include further discussion and details on the thickness determination in a section of the SI entitled "Morphology and estimation of sample thickness", including the details requested.

R2#09

> *"Supplementary Fig 3: Please explain the annotations - orange and blue arrows - in the caption. What was the electron dose (fluence) used in the initial TEM image?"*

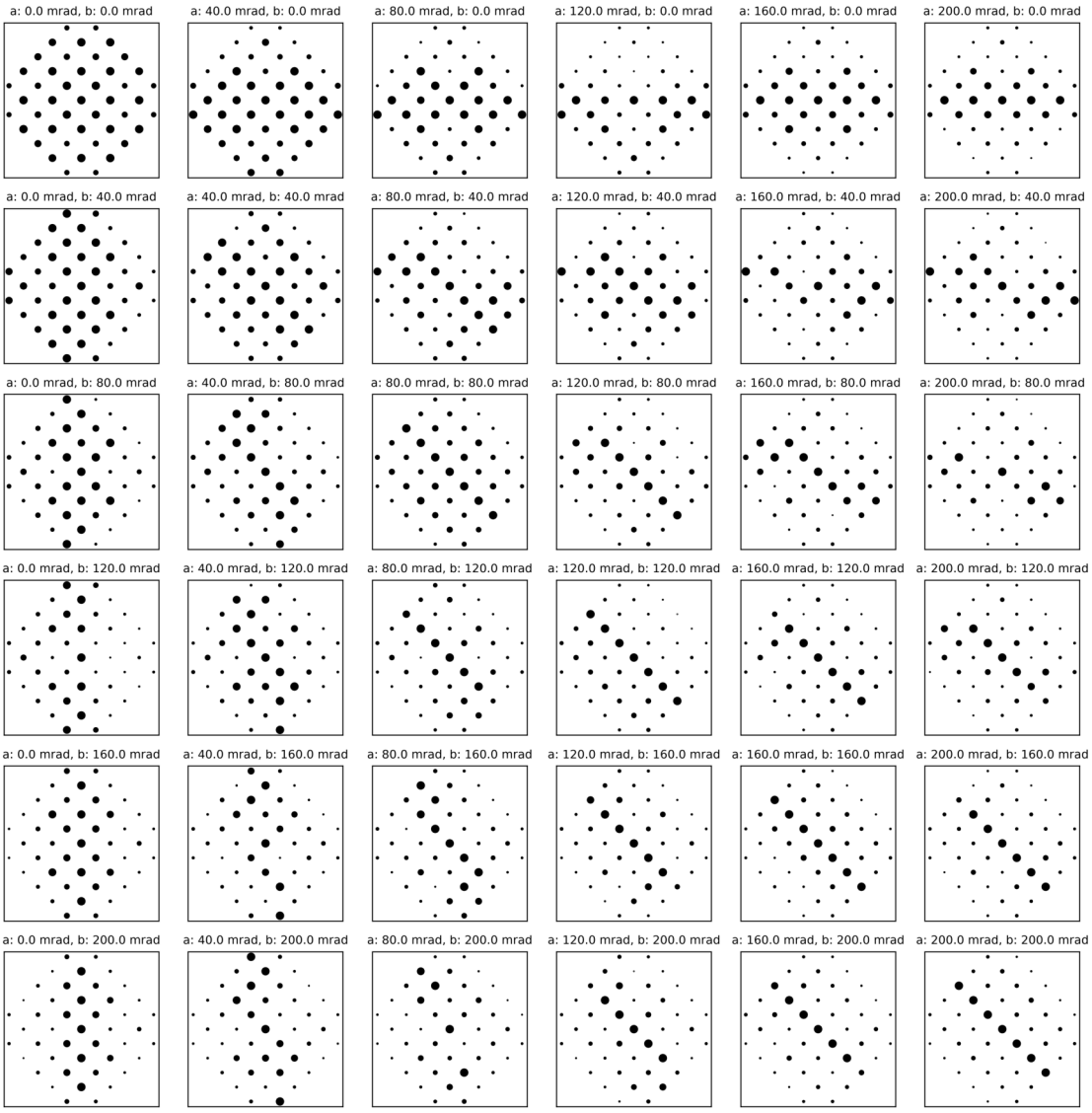
We have added these in the revision.

R2#10

> *"From a single orientation diffraction can you discount a partial tilting of the tetragonal phase from viewing down the [001] direction to viewing down [100] or [010] rather than a transformation to orthorhombic? If this conclusion is based on the literature then this needs to be cited.*
>
> *What are the measured bond lengths from the diffraction patterns - how do they correspond to literature values from e.g. xray studies?*

Yes, we can distinguish a partial tilting of the tetragonal phase from a transformation to orthorhombic. Tilting only changes the intensity of the diffraction spots while structural changes introduce new ones.

RFig. 2, shown below, is a simulated diffraction tilt series of the tetragonal phase illustrating that tilting only changes the intensity of the spots. As shown in SFig. 6 however,



RFig. 2. Simulated diffraction tilt series of the tetragonal phase of $MAPbI_3$. The labels 'a' and 'b' represent the tilt angles along the [010] and [100] directions, respectively, with the unit being milliradians (mrad).

new diffraction spots are observed as the material damages that match the spacings of the orthorhombic phase. A simulated diffraction pattern for the orthorhombic phase is shown in SFig. 9 alongside that of the tetragonal phase.

Additionally, we can directly distinguish between the two phases from high-resolution images, as shown more clearly now in SFig. 23. In the tetragonal phase, the PbI_6 octahedra in alternating layers twist in opposite directions, while in the orthorhombic phase, they twist in the same direction. This twisting behavior of the PbI_6^{2-} octahedra is can be seen directly

in the high-resolution images. Moreover, during the damage process, we observe a gradual increase in the intensity of the (210) plane of the orthorhombic phase in the power spectra (SFig. 22). This trend aligns with the experimental diffraction results (SFig. 6), where the additional (210) spots intensify progressively rather than appearing abruptly.

Based on these simulations and direct observations from high-resolution structural imaging, we conclud that the transformation to the orthorhombic phase is the most plausible explanation. This conclusion is supported by literature reports on similar transformations (10.1021/acs.jpcc.3c01546; 10.1021/jacsau.3c00060).

We have also included quantitative bond length measurements extracted from the diffraction patterns and compared them with values reported in the literature for both the tetragonal and orthorhombic phases of $MAPbI_3$ (doi.org/10.1038/s41563-024-01921-0; doi.org/10.1038/nmat4271).

R2#11

> *"Line 101:"Despite the lower irradiation intensity of TEM, its continuous plane wave illumination actually results in significantly more cumulative damage. Surprisingly, using an extremely rapidly scanned probe the sample exhibits over 10 times the stability compared to a parallel electron beam. Therefore, choosing a focused probe and fast scanning holds significant potential to extract a greater amount of information before damaging the sample."*
>
> *"dose rate" dependence has been investigated in Ultramicroscopy 232 113398 (2022) with theoretical interpretation in Ultramicroscopy 240 113568 (2022). These findings explain the greater damage in plane wave illumination."*

Thank you, we agree that these are good references to point to regarding potential reasons for the greater damage rate under plane wave illumination. We now cite them and include significant discussion based on their findings and model in both the main text and a new SI section entitled "Discussion of MAPbI3 dose tolerance in STEM and TEM".

R2#12

> *"Also Figures 3-5 in supplementary information need accompanying text as there is not sufficient information in the main text or in the supplementary figure captions to*

explain to the reader how the data demonstrates the authors claims that there is less damage with a scanning beam than with the plane wave illumination."

We have overhauled the SI, adding significant discussion on the various topics which we lack the space to expand on in the main text. This includes fixing overly terse captions and adding multiple sections of discussion on this particular topic.

R2#13

"Line 114: "...apart from 1 or 2 pixels,"

There are exactly 2 events shown in fig 1c - though it looks like 4 px counts, the upper event having counts in consecutive horizontal and the lower event counts in consecutive vertical pixels.

I can accept that each discrete event is due to one incident electron - with two px being illuminated due to the non-perfect modular transfer function of the detector - though for an event based camera like the timepix it should be possible to account and correct for this. This does not align with the statement "a single CBED yields only ∼1 e-" (line 113). The displayed frame clearly shows 2 electrons. Calculation of the expected number of events per frame for 0.2 pA and 500ns frame time gives 0.6 electrons per frame and 0.3 pA gives 0.9 electrons per frame. It is possible to accurately calibrate your beam current from your dataset. Either your beam current is not calculated correctly or the diffraction pattern you show is not representative of the data set."

Yes, the displayed diffraction pattern showed two hits, indeed one more hit than the average per probe position. We now show a diffraction pattern with only one hit in Fig. 1c to be in line with the average for that dataset, and have revised the text to more clearly state that one hit is the average.

We have indeed published a declustering aglorithm for the Timepix3 in Jannis et al. (10.1016/j.ultramic.2021.113423), however this yeilds no significant benefit for ptychography.

We have also revised the text to state that the probe current is 0.3 pA rather than the range we specified previously, with more precise values provided for the different datasets in a new

table in the SI (Supplementary Table 1).

R2#14

> *"Figure 1: "Color bars are from 0 to 1, and from 0 to 0.0015, respectively."*
>
> *What are the units of the sum CBED (or PACBED) pattern, why is the scale from 0 to 0.0015. This is not the number of recorded electrons as is shown in the individual CBED pattern. This needs correcting."*

We have removed the colour bar from Fig. 1c, which is unnecessary for a single hit, and described in the caption that it is a single hit.

The 0 to 0.0015 value range previously given for Fig. 1d indicated the range of an average number of hits for each pixel. We now indicate the values for the sum of the hits, i.e. a range from 0–144, where 144 is the maximum number of hits in a single pixel over the whole scan.

R2#15

> *"Line 120: "While the scattered electron signal beyond 40 mrad is weak, the radial distribution over which the high-angle ADF (HAADF) detector integrates is broad enough to capture signals from heavy atoms like Pb simultaneously with the 4D-STEM."*
>
> *What is the inner angle of the ADF detector? I assume it is beyond the outer angle of the timepix camera - this should be stated explicitly.*
>
> *What is the difference between the intensity in the BFD and that in the (extrapolated) region of the ADF detector from the PACBED image? From this value you can calculate the average number of electrons per unit angle and therefore know how many electrons you would expect to measure per probe position in the ADF detector. I'm surprised that with just two electrons in the BFD you get any signal at all in the ADF - this should be well below the Poisson noise limit - unless you are greatly oversampling in your probe space - what is the probe step size? Please address these comments in the text."*

The inner angle of the physical HAADF detector is around 44 mrad, just outside the range

collected by the Timepix3. However, the LAADF uses the data from the Timepix3 using an inner angle just outside the 13 mrad of the bright field disk.

Yes, the probe positions are oversampled, using a very fine probe step size of 0.03 nm, which kept the probe moving as much as possible. Therefore it seems natural that there are very few hits per probe position.

We now list the probe step sizes for different datasets in Supplementary Table 1 and the angular ranges used for the different signals in Supplementary Table 2.

We do not know how many electron hits occurred outside the Timepix3 detector as the HAADF was not calibrated, and the extrapolated line in Fig. 1 referred to is for illustrative purposes only. We have however calculated that the amount of hits on the Timepix3 drops by approximately 4% when scanning on the ultrathin $MAPbI_3$ compared to scanning in vacuum. We now note this in the SI, in the section entitled "Probe current for ultra-low dose work", regarding our calculation of the experimental probe currents and doses.

We have also replaced the original Fig. 1e with a profile showing the average number of electrons per unit angle.

R2#16

> *"Line 127. "The robustness of non-iterative ptychography to noise allows for phase image reconstruction with minimal signal 20."*
>
> *Reference 20 [Pennycook TJ, Lupini AR, Yang H, et al (2015) Efficient phase contrast imaging in stem using a pixelated detector. part 1: Experimental demonstration at atomic resolution. Ultramicroscopy 151:160–167] does not show data with minimal counts on the detector - in fact the data presented in this reference is acquired at 30 frames per second, though the beam current is not stated. A better reference is O'Leary et al Appl. Phys. Lett. 116, 124101 (2020) which was the first demonstration of ptychography using 'binary' data. "*

Indeed, the citation was for dose efficiency rather than actual reconstructions at low dose. The text has been revised, including the citations, and the O'Leary reference using the 1-bit mode of the medipix3 camera has been cited in the SI discussion. Of course, strictly

speaking, digital data tends to be binary even if not collected in a 1-bit mode, but we agree it is a good reference.

R2#17

> *"Line 129: "However, iterative ptychography often struggles with such an extremely low number of electrons per CBED pattern due to the difficulty in guiding the convergence."*
>
> *Is this statement based on evidence in the literature. If this is based on previous work in the literature please provide a citation. If not then some evidence from your study needs to be presented to support this statement."*

The difficulty of correctly converging iterative ptychography with low counts per diffraction pattern is well known in the community, but is to our knowledge not very well documented in the electron microscopy literature. This likely stems from the tendency to publish with an emphasis on successes rather than point out where methods have issues. This is something we aim to help address more in future technical publications on optimising iterative ptychography for low dose applications. We can point to our own previous publication (doi.org/10.1063/5.0101895) showing an example of iterative ptychography failing to converge to anything useful for a simulated focused probe dataset at a very high dose of 10^5 $e^-/Å^2$. However we are confident we could converge this dataset now at lower doses by better tuning the convergence parameters for these.

Therefore we have introduced a section in the SI called "Convergence of iterative ptychography at ultra low doses" where we discuss the selection of the convergence parameters of ePIE and illustrate how the algorithm can diverge. As we discuss, one typically needs to reduce the update step size as the diffraction patterns become sparser, which makes convergence slower. The original ePIE paper used step sizes of a=b=1. In the SI we show that using a much smaller a=0.05 b=0.01 converges at 1000 $e^-/Å^2$ but diverges at 24 $e^-/Å^2$, requiring an even smaller set of parameters (SFig. 14).

We have also modified how we refer to this issue in the main text, stating that compared to direct ptychography:

"iterative ptychography, including ePIE, is much slower, and convergence can be challenging

under these conditions (see SI)"

R2#18

> *"Line 139: "Through the iterative and non-iterative phase reconstruction methods, achieving atomic-level phase and ADF imaging becomes possible."*
>
> *It is not clear how the ptychographic reconstructions enable atomic resolution ADF imaging. Please provide more detailed information."*

Indeed this sentence was malformed. The intent was to refer to simultaneous ADF and ptychography. The error has been addressed in our rewrite.

R2#19

> *"Supplementary fig 6:*
>
> *Subfigures b) and f) show the aligned sum of single frames similar to subfigures a) and e). There are almost no discernible features in a) or e). I'm not convinced you can reasonable align subsequent phase images which are so noisy with no clearly discernible features. Please provide further details on how this was done.*
>
> *Sub figures c and g) - are these simply summing diffraction patterns, with no alignment? This is not clear in the caption. If this is the case, how are inconsistencies in probe positions on subsequent scans accounted for? Please provide additional information."*

We agree the presentation was lacking, and now explain the drift correction in a full section of the SI entitled "Dose fractionated imaging and drift correction".

The simulated images in the former SFig. 6 (SFig. 18) compare the results of performing ptychography on perfectly aligned 4D data or summing the perfectly aligned phase images from each individual scan. Note that the individual scans used in this figure are lower dose than what was used experimentally.

R2#20

> *"Supplementary figure 7:*

There are some phase wraps in the reconstructions resulting in the heavier element columns appearing as bright doughnuts with a dark center - this can be corrected for using a standard phase unwrapping algorithm. This problem is seen in all subsequent ePIE reconstructions and makes them significantly more difficult to interpret as well as precluding the possibility of an quantitative phase analysis. Please resolve this issue or at the very least have a detail discussion highlighting the presence, cause and reasons why it can not be resolved."

We now use our phase offset method of contrast reversal correction to counteract the dips on the Pb-I columns as we discuss in detail in the section of the SI entitled "Contrast reversals and their counteraction". Note that these contrast reversals are expected from simulations, including noise free simulations, and only occur on these heavy columns. They cannot be removed by a standard phase unwrapping algorithm as they are not simple phase wraps. The range of phases in such images is generally rather small on the range of 2π, and the phase wrapping occurs in reciprocal space rather than the real space images.

R2#21

Supplementary figure 7:

"For c) and f) how are the inconsistencies in probe positions accounted for when the diffraction data is summed? If the global drift / scan distortions are corrected for by comparison with the individual reconstructions there will still be a variable probe 'jitter' within each scan. The standard approach to corrected this is to use a probe position annealing technique (Ultramicroscopy Volume 120, September 2012, Pages 64-72) - this would not be possible on the summed diffraction data. Please provide more detail on the summed reconstruction process."

The approach to drift correction is the same as in standard rapid scanned STEM. Probe jitter is not significant for the purpose of revealing the lattice, and there is no need for probe position annealing.

We now provide a full section in the SI entitled "Dose fractionated imaging and drift correction" discussing the drift correction in detail, including a demonstration of the lack of benefit provided by probe position annealing.

Probe position annealing is more beneficial when the probe step size is much larger than used here, such as in a defocused probe configuration.

R2#22

> *""In order to realize the sub-pixel alignment, we increase the probe position sampling by 2 (more is possible, but that would need much more memory to load and process 4D-STEM data). "*
>
> *What does this mean? Are you collecting addition probe positions (or using additional probe positions from the acquired data)? How/why does this enable sub-pixel alignment. Please provide more detail."*

This was referring to the sub-pixel drift correction previously referred to in the main text, in which the alignment grid is finer than that of the images being aligned. This is common in alignment of rapid scan STEM images.

We have revised the wording in the main text and written a new section describing the drift correction, including this detail, in the SI called "Dose fractionated imaging and drift correction".

R2#23

> *"Please show reconstructed probe functions alongside the reconstructed images (or in the supplementary information). It is difficult to assess how realistic the object reconstruct is without being able to examine the probe function."*

We now show representative examples of the probes produced by ePIE in SFig. 29.

R2#24

> *"Line 152: "Whereas the phase images reveal the MA+ columns..."*
>
> *It is not clear that this is the case - please mark the location of the MA+ on the figure and provide a description in the caption."*

Regrettably, many of the images in the original manuscript were displayed improperly making them overly dark and high contrast. We have addressed this in our revision, and added

annotations and models to make the locations of the MA columns clear.

R2#25

> *"Line 153: "The Fourier transforms (FT) of the phase images..."*
>
> *Strictly speaking these are two dimensional power spectrums which are calculated using a power spectrum."*

We now use the power spectra terminology.

R2#26

> *"Figure 2:*
>
> *It looks like the ADF image has a significantly smaller pixel size that the ptychographic reconstructions. It should be possible to bin (sum pool) the ADF image to the same px size as the ptychography reconstructions. This will improve the signal to noise of the ADF image. With the addition of common smoothing techniques (e.g. Gaussian kernel filter, two-dimensional Savitsky-Golay etc) it may be possible to improve the quality of the ADF image to similar to the ptychographic reconstructions. Please provide this comparison to convince the reader that the ptychographic reconstructions really do give an improvement in signal to noise and resolution."*

We apologize for the poor presentation in the previous submission with the aforementioned display issues. It should now be clear that the ptychography provides much higher clarity in the revised figures, as is to be expected from the dose efficiency of ptychography.

However we now present Gaussian filtered LAADF and ePIE images in SFig. 32 alongside additional discussion in the section of the SI called "Experimental comparison of different imaging modalities". Gaussian filtering appears to work better than Savitzky-Golay, so we only showed the results of the Gaussian filtering in the revision. Although the filtering smooths the signal, it is unable to make up for the overall weaker signal of the LAADF comapred to the ptychography, and in SFig. 32 we point out a region of columns at the edge of the material that are visible in the ptychography but are not visible in the LAADF. Most likely these are underoccupied and therefore not visible above the noise in the LAADF.

Regarding the pixels sizes, the SSB ptychography and ADF images have the same pixel sizes, while the ePIE ptychography images are only about 3% larger. This does not seem worth binning the ADF for.

R2#27

> *Figure 2: "Again the ePIE reconstructions show what looks like phase wrapping at the centre of the heavy atom columns (as do all subsequent ePIE reconstructions)."*

As noted in previous responses, these are the expected contrast reversals which have now been corrected using our phase offset method of contrast reversal correction.

R2#28

> *Figure 2: "The atomic position markers in e) are poorly placed and miss the atomic columns on the bottom row. They are also not equally spaced - particularly the bottom row."*

We have addressed this in our revised figure.

R2#29.

> *Figure 2: "Subfigure h), is this measure of information transfer a reliable gauge of usable image resolution? Please comment. What is the probe step size in this data? It is common for their to be features in the 2D power spectrum corresponding to probe step size. Is this the full power spectrum or has it been cropped? If this is the full power spectrum then there is information beyond Nyquist frequency which should not be possible. If cropped please state that this has been done. There seems to be information beyond the 1.1A marked on the figure - in the corners of the power spectrum. Why are these not considered"*

We apologise for the poor presentation.

The probe step size for this dataset is 0.03 nm, as we now state in Supplementary Table 1. We confirm that the 2D power spectra have been cropped for the clarity as we now note, focusing on the region relevant to our analysis.

Previously the FTs were performed on the data with dips in the centres of the Pb-I stites due to the contrast reversals. These indeed produce spots at higher resolution than the actual useful information transfer, as seen beyond the 1.1 Å mark. This is indeed a good reason to remove the contrast reversals, as we have now done. The spots at higher resolution than 1.1 Å in power spectra of the corrected images have disappeared leaving a spot at 1.1 Å.

We believe the updated power spectra are now a reasonable indication of the resolution, although of course the more important fact is that the significantly wider spacings of the atomic columns are well resolved.

R2#30

> *Figure 2: "'A' is used to denote Angstrom - please use the correct Angstrom symbol."*

This has been fixed.

R2#31

> *Figure 2: "The phase of the SSB reconstruction seems strange to me. The Pb columns should have the highest phase shifts, it is not clear that thy do. In fact in some places the MA+ column seem to have the highest phase shift. Please comment on this."*

This is again the result of poor image display and contrast reversals on the Pb containing columns. We have applied the phase offset in the revision as now described in the SI. Note that the Pb-I and I and MA columns can appear relatively similar in the SSB images, including in simulations."

R2#32

> *"Supplementary fig 9. d): Please explain the yellow markers in the caption. f) should be labled orthorhombic not orthogonal."*

We now explain that the yellow markers indicate newly emerging spots belonging to the orthorhombic phase, and have fixed the misspelling of orthorhombic (SFig. 9).

R2#33

> *"Supplementary figure 10.: What Fourier filter is used to create the images in b and*

d. The use of Fourier filters, in which only specific, user defined frequencies from an original image are passed to the filtered image can create significant artefacts in images."

To eliminate the possible influence of artifacts, we replaced the original figure with the unfiltered image, now presented as SFig. 23.

R2#34

"Addition information is required to explain the classification of regions as tetragonal and orthorhombic. b and c show no immediately discernible difference. Do the arrows highlight positional variation? If so this needs to be explained in detail and quantified. The coloured dots on the image b and d need to be explained in the caption."

The transition to orthorhombic is now discussed in detail in the SI, including in a section entitled "Evolution of pristine MAPbI3 under electron irradiation" that contains a new figure (SFig. 23) in which the changes are clearly apparent in the images, which are also better illustrated with accompanying models.

R2#35

Supplementary figure 11, caption: "The resolution achieved by the ePIE phase image is significantly higher than that of ADF images."

How much higher? Please quantify."

We now use annotations on the power spectra in the updated figures to indicate the resolutions determined from the highest frequency spots apparent in each method. We have added a section to the SI entitled "Experimental comparison of different imaging modalities" in which we discuss this in more detail.

R2#36

"Again Supplementary figure 11 k) shows phase wrapping on numerous atomic columns. There also seems to be strange phase contrast with heavy atomic sites sometimes appearing brighter (larger phase shift) and sometimes not - though this may be due to

phase wrapping. This needs to be resolved in the presentation of the data or explained in the text."

Another instance of the contrast reversals and regrettably poor presentation in the initial submission. This has been addressed in previous responses (see eg R2#1). Noise does however naturally cause some variation in the brightness of the columns.

R2#37

"Line 162: "Ultra-low-dose 4D-STEM imaging techniques provide atomic-resolution HAADF, LAADF, iCOM, ePIE, and SSB images (Supplementary Fig. 11-12). Our LAADF and phase images provide enhanced visualization ... "

These comments are misleading. The HAADF is not from the 4DSTEM data set and the LAADF could be collected using a standard monolithic detector. If your claim is that the alignment between scans is only possible due to the higher signal to noise of the ptychographic reconstructions which then enable the summation of the virtual-LAADF and HAADF then this needs to be made explicit."

We agree the phrasing should have been better. The point is more about the capabilities of focused probe 4D STEM to provide all these signals, but indeed this was poorly presented and the section containing this text has been rewritten in more precise and clear language.

R2#37

"Supplementary Figure 13: The measurements on d) and e) are unclear and the labelling is too small.
The C-N from the MA is shown at a single atomic column position. At all other positions in the reconstructed image they are not resolvable. This makes me sceptical about the claim the the C-N atoms within the MA molecule can be resolved. Why is this only shown at one atomic column position? Additional examples showing resolution of the C-N atoms need to be presented or a reason why only one example was found needs to be discussed in the text."

Indeed the presentation was poor, including the display of images themselves. We now

provide a section in the SI "Imaging MA molecules in detail" discussing the possibility of observing the rotations, including a comparison to simulations and more examples from the experiments.

R2#38

> *"Line 181: "By the 10th to 12th scans, Region I transitions from its original perovskite structure into lead iodide,"*
>
> *This is not made clear in the figure - the reader needs to be informed what the structure of PdI_2 is and the change needs to be highlighted on the figure with for example an atomic model overlay, or measurements of bond lengths."*

We agree the presentation was lacking. Furthermore, after reexamining the data we believe this region is actually more I deficient than initially thought and most likely transforms into Pb metal rather than PbI_2. Additional images of the region and its evolution are displayed in SFigs. 40 with power spectra. Both the contrast and lattice spacings of this region match those of Pb metal after damaging. Models comparing the lattices of $MAPbI_3$, PbI_2 and Pb metal are shown in SFig. 7 alongside simulated diffraction patterns. An annotation has also been added to Fig. 3 to indicate the location of the transformation.

Furthermore, additional discussion of the observed transformation has been added to the SI, including in the section "Termination of the edge of $MAPbI_3$ and its evolution under electron irradiation".

R2#39

> *"Line 187: "virtual ADF"*
>
> *Previously this has been referred to as LAADF - please remain consistent throughout the document."*

This has been fixed.

R2#40

> *"Line 187: "The averaged images and intensity profiles(Fig. 3d-3f) show Region I has iodine defects and a lead-terminated edge, while Region II has minimal defects and terminates with iodine and MA. Models illustrating these observations are shown in Fig. 3g and 3h."*
>
> *I don't agree with this interpretation. The ePIE and SSB averaged reconstructions, shown in Figure 3d clearly show iodine termination, all be it with slightly lower intensity than in Fig 3e. it is possible that this is due to a single iodine atom terminating, thought this would need to be backed up by simulation. Due to the phase wrapping problem (causing dips in the centre of the Pb columns) it is difficult to quantitatively compare these results. I don't see any evidence of MA termination in the ePIE averaged reconstruction of fig 3e). I cannot agree that the imaged presented in Fig 3d and e conclusively demonstrate the atomic models shown in g and h."*

We agree that terminating edge layer is not a Pb terminated edge. The previous description arose from considering that edge vacancies expose the next layer back. We now state clearly that the edge is an I-MA layer. We have heavily revised Fig. 3 and the text describing it, and also added additional discussion and figures in the new SI section "Termination of the edge of $MAPbI_3$ and its evolution under electron irradiation". It should now be clear that that the edge is indeed terminated by an I-MA layer, with the improved display of our images and accompanying simulations. This termination is particularly apparent in the lateral unit cell averaging shown in Fig. 3b. Here the I-MA terminating layer is brighter in region 2 than region 1, indicating there are more vacancies in region 1.

R2#41

> *"Line 192: "The images show a progressive reduction in MA+ at the edge, commencing from the initial scan, underscoring the susceptibility of MA..."*
>
> *I think this is incorrect. I can see no evidence of MA termination if Fig 3 i)-1. The edge seems to be Iodine terminated."*

The updated figures with corrected display contrast should address this, along with our additional simulations. The MA columns in the I-MA terminating layer are particularly apparent in the lateral unit cell averaged images, such as that of region 2 in the new Fig. 3b, and in our updated figures in the SI.

R2#42

> *"Line 200: "By the 7th scan, PbI_2 began forming."*
>
> *Where/what is the evidence of this? Which figure is it shown in? Please provide additional information"*

This refers to Fig. 3. As stated in R2#38, we agree the presentation was lacking on this transformation, and have revised to show how this is actually most likely Pb rather than PbI_2. This includes rewriting the text describing the transformation, and it should be clear now which figures are being referred to. We have also added significant description and figures to the SI describing this transformation.

Additional discussion and figures describing the observed transformations have been added to the SI, including in the section "Termination of the edge of $MAPbI_3$ and its evolution under electron irradiation".

R2#43

> *"Line 201: "Supplementary Fig. 18), we observed that most edges are terminated with I- and MA+ 202 , which is consistent with the DFT calculations "*
>
> *Again I don't agree with this interpretation. I can see very little evidence in Fig 18 of MA termination. It appears almost exclusively Iodine terminated."*

The updated figures with improved display contrast along with our simulations should address difficulties in seeing the MA in the I-MA terminating layer (SFigs. 35, 36, and Fig. 3c). There are MA vacancies in the layer, but it should now be clear MA is present at the edge.

R2#44

> *"Line 205: "These observations underscore the rapid collapse of the crystal structure*

induced by defects, emphasizing the substantial impact of edge structures on material stability."

What is the structure of these edge defects which induce rapid collapse? Please add additional information."

As we now describe better in our overhauled text, I vacancies appear to be the main cause of instability at the edge.

R2#45

"Line 217: "However we have also identified additional forms of defects, specifically, Pb defects and I defects, which are clearly visible in the phase image..."

The data presented is not sufficient to support this conclusion. Without comparison with simulated phase or projected potential images it is not possible to evaluate whether the experimental data supports the proposed atomic models."

We have now correctly labelled the simulations we included previously and have included additional simulations in our revision. The simulated phase images, including those of vacancy containing structures, allow to interpret the phase images in terms of structure.

R2#46

"Line 228:"...but it forms away from the defects..."

Supplementary figure 19 does not conclusively show this. Although the yellow arrows do in some instances point to regions of the sample in which the MA column position is similar intensity to the Pb-I columns it is not clear that this is PbI_2. At the very least the reader needs to be informed on the structure of PbI_2 and what features in the images confirm that this has been identified."

We have rewritten our description of the defects in the interior regions, and include a description of the PbI_2 structure in the SI, including a model and simulated diffraction pattern in 9. This includes the spacing that we use to identify it from locally acquired power spectra

as we now describe in more detail.

We have also added additional discussion in the SI section "Effect of internal defects on stability".

R2#47

> *"Line 235: "Our observations show that I defects are unstable centers, while Pb defects are stable ones."*
>
> *I do not believe that the evidence supports this claim. The study of edge termination only showed one example in which it appeared as if the Pd-I terminated edge was destroyed faster that the Iodine terminated (figure 3). For bulk defect one example (figure 5) is shown which appears to be both Pb and I deficient and then grows more or less isotopically with increased electron dose. At no point in the paper is there a comparison between the stability of Iodine and Lead defect sites."*

We have heavily revised our description of the effects of the defects and included additional data to show that the prevalence of I vacancies correlate with an increased rate of damage. This includes discussing the interior defects shown in Fig. 4 and others in terms of their relative occupancy, estimated net ionic charge, and the rate at which they damage. So for instance the defect in Fig. 4a contains a higher concentration of I vacancies than Pb vacancies, and rapidly expands. Whereas the other two defects, centred on Pb vacancies, have proportionally more Pb vacancies and are as stable as the pristine bulk material. Other examples are given in the revised SI, particularly in the section "Effect of internal defects on stability".

R2#48

> *"Supplementary figure 19*
> *Typographic errors in the caption PhI_2 should be PbI_2."*

These typographic errors have been addressed.

Referee Response

We thank the reviewers for their constructive criticism, and respond to their comments point by point below.

Referee 1

> *"The authors have made substantial revisions in this version and added significantly more data in the Supplementary Information to support the claims and illustrate the technologies in more detail. The image quality as well as the presentation quality have also been improved. The authors have successfully addressed my questions in my previous review to my satisfaction. I also noticed that the contrast reversal issues of heavy atomic columns in the previous version have been corrected using the phase offset method, which makes the images more legible.*
>
> *In general, I am satisfied with the revision. However, I request the following minor revisions before the manuscript proceeds to publication:"*

We thank the reviewer for their positive evaluation. We are happy that they are satisfied with the revision overall, and have addressed their remaining minor comments as detailed below.

R1#01

> *"Abstract: The tone of the abstract could be elevated to match the achievement of this work that qualifies its publication in Nature. Important achievements such as the first-time observations of atomic-resolution edge structures and dynamics using the lowest dose could be emphasized, echoing the relevant statement in the main text and Conclusion."*

We thank the reviewer for recognising our achievements as important and worthy of publication in Nature.

We have revised the abstract as requested.

R1#02

> *"Section 2.3 – Interior region defects: This section needs to be rephrased to make the main text more logical and self-contained, though readers could get the message the authors are trying to convey by referring to the relevant sections in the Supplementary Information. After mentioning "Defects within the interior of the crystal can also strongly affect stability," it is better for the authors to first mention that they found two types of defects associated with different net ionic charges that show different stabilities, and then elaborate on the two types of defects."*

We thank the reviewer for pointing out that we could improve the language here. We have revised the section accordingly. We also rectified an apparently missing reference to the defect in Fig. 4a, representing the first class of defects. We believe this greatly improves the readability of this section.

R1#03

> *"Supplementary Information, Page 32, last paragraph: "the MA columns at the edge appear to be slightly closer to the I columns in the Pb-I columns in the next layer inside the material" – What does this exactly mean? It is not clear, though the readers might have the right guess. I recommend adding two pairs of vertical lines in the line profile to illustrate the distance difference, instead of just saying "appear to be.""*

Thank you for the suggestion. We have added two pairs of vertical lines in SFigs. 39, which highlight that the distance between the outermost MA and I atoms is smaller than that between MA and I atoms in the interior region.

R1#04

> *"Line 94, It is not clear what the authors are trying to say here; "With the small probe step size we used to keep the probe moving as much as possible". Please make some explanation or rephrase it."*

This phrase was referring to our very fast scan using a very short dwell time, which was motivated by this material appearing to damage less with a very fast continuously moving probe.

We have removed this statement and instead now note in the previous paragraph that our probe position grid is dense, which we believe is more appropriate and improves the readability.

R1#05

> *"There are some errors that need corrections:*
> *a) Fig.2 Please also add the annotation to the color circles/dots as in Fig.1."*

We have added text annotations that match those of the colour circles in Fig. 1.

> *"b) Fig. 3 (f) should be (e); Suggest to add scale bar in every experimental image here."*

We have fixed the reference to a nonexistent part (f) in the caption. Thank you for spotting this.

We feel that supplementing the present scale bar with additional scale bars on every image of the figure would block too much of some of the images, be distracting, and add very little information or readability.

> *"c) SFig. 46 (d), the dose should be 12.6×3;"*

Thank you, we have fixed this in the revision.

> *"d) The last sentence of SFig. 47 annotation needs to be corrected to fix grammatical errors; "*

Thank you, this has been fixed in the revision.

Referee 2

General comments:

> *"In this work the authors present a study of the structure of defects and induced electron beam damage in methylammonium lead iodide (MAPbI), an organic-inorganic perovskite with promising semiconducting properties for potential applications including*

in next generation solar-cells. The authors show that by using electron-ptychography (which is known for its high imaging efficiency) the atomic structure at edges and defect sites can be determined and that the presence of defects increases the rate of crystal breakdown under illumination with the electron beam.

The images presented represent the lowest electron dose ptychographic atomic resolution imaging to date (to the best of my knowledge) and the technique has been applied to a technologically important class of materials which are challenging to image at these resolutions. As the authors acknowledge bulk defects and grain boundaries of MAPbI have previously been imaged at atomic resolution using LAADF STEM at doses of 60-100 e/A^2 [Rothmann, M. U. et al. Science 370, eabb5940 (2020).] Other beam sensitive materials have previously been studied using broad beam TEM illumination at doses of a few 10's A/^{2} [Zhang, D. et al. Science 359, 675–679 (2018).] equivalent to the doses used in this study.

The main focus of the paper is to identify that the electron beam damages the samples significantly quicker at iodine deficient edge locations and at bulk defect sites. This is a generally unsurprising result. The authors do not provide a detailed statistical analysis of edge structure nor provide a detailed study of various bulk defect structures which would be of general interest to the halide-perovskite community and inform on future material understanding and fabrication.

The data presented is of a high quality with and analysis is generally sound. I have some concerns over the attempts at phase quantification (this has been notoriously challenging for the electron microscopy community for many years) and the subsequent assignment of "net ionic charge" states. These concerns are detailed below.

In my opinion this study does not reach the high bar for general interest which is required of a publication in Nature and I cannot recommend for publication. I recommend resubmission to a journal with more focused readership after addressing the specific comments below.

We thank the reviewer for recognising our work as the lowest dose ptychography to date, our data of high quality, and our analysis generally sound.

However, their point that conventional broad beam TEM can be used at similar doses for other materials misses the crucial point that the halide perovskites damage far faster in TEM than in STEM, as we discuss at length. The damage rate we find in our TEM experiments agree with those of previous studies, and our study would not have been possible in plane wave TEM. Despite the great interest in these materials, no comparable atomic resolution TEM images have been published of the edges of the organic halide perovskites.

Moreover, our study would not have been possible in conventional STEM either, as the contrast of the ultrathin material is too weak to resolve all the atomic and molecular columns in ADF imaging. The significantly greater dose efficiency of ptychography was required for this.

To our knowledge, no previous study has been able to provide the level of detail we provide here of any halide perovskite.

Our study provides important findings regarding the damage mechanisms of halide perovskites that previously were not accessible experimentally. By demonstrating that this is possible with ptychography, our work opens the door to further, more detailed studies of halide perovskites, and indeed other beam sensitive materials that damage in a similar way. Such very rapidly scanned 4D-STEM may also facilitate in situ studies with ptychography, where time resolution is important for capturing dynamics.

We address the specific comments of the reviewer below, including those regarding the quantification and net ionic charge states.

Specific comments:

R2#01

> *"Line 56 - "less than one thousandth the dose typically used for stable samples.[14, 17]" This statement is misleading. Reference 17 is a demonstration of atomic resolution TEM imaging of MOF samples at cumulative doses of 10-20 e/A^2, perhaps double the approximately 6 e/A^2 dose budget for MAPbI3 stated in the text."*

We do not see how this statement could be misleading as the statement is clearly true – the doses needed to image halide perovskites are indeed less than one thousandth the doses

used for stable samples that are more robust to the electron beam. The TEM based MOF imaging the reviewer refers to is also a very low dose imaging indeed, so clearly is not the comparison we are making.

We can only speculate that, unlikely as it seems, the reviewer maybe confused by the references we cited here, which were cited to emphasize the low electron dose tolerance of organic perovskites, not as examples of imaging robust materials. In addition to the MOF the reviewer refers to, reference 17 also imaged a similar perovskite $MAPbBr_3$ at a dose of 11 $e^-/Å^2$. We have therefore repositioned references 17 and 14 to the earlier part of the sentence for better clarity.

R2#02

> *"Line 88 -In fact using multiple scans the material withstands between five to ten times the dose than in TEM (see SI)" The authors show that, using broad beam electron diffraction the MAPbI samples damage at electron doses of a few 10s e/A^2 (supplementary figure 6). The analysis of this broad beam diffraction data is fundamentally an average of the whole illuminated area (a large crystal) which likely includes a large number of defects and unstable edges. In fact, the major focus of the manuscript is that defects are susceptible to damage at these low electron doses. In my opinion the data presented in the supplementary information does not provide convincing and conclusive evidence that a low dose high resolution TEM study would be unable to image defects and edges of this material."*

We do not state that TEM would be unable to image the defects and edges of the material. However, it is clear that it would be far more difficult to do so with sufficient clarity for the purposes of our study. This is because critical dose studies show that the material damages far more rapidly in TEM than in STEM, both in the literature and our own studies.

Testing the critical dose of a material using reciprocal space analysis using an average of the whole illuminated area is a very well established standard used in both TEM and STEM. This method is used in study after study, and our results match the results of previous papers extremely well. Moreover, we emphasise *both* TEM *and* STEM critical dose studies use averages over similar large areas. Therefore our comparison of the critical doses is fair

and unbiased, and explains why it is much more difficult to image this material in TEM than STEM, especially at its especially fragile edges and defects.

As we note that the reviewer only notes our critical dose study for TEM in this comment, we would kindly point them to our entire section of analysis of the damage rate with STEM entitled "Damage rate with a convergent scanned probe". We have examined the text around line 88 of the main text to see if we can improve the clarity, but we see we refer to our comparison of the dose rate in STEM and TEM, with the relevant SI figures explicitly cited, only two sentences prior. Therefore the present language seems sufficient.

We have also added a section to the SI called "Discussion of $MAPbI_3$ contrast in TEM" discussing some of the additional difficulties of plane wave TEM imaging we did not previously discuss in as much detail, including the use of the latest focal series reconstruction techniques.

We also note that in the initial round of reviews, the reviewer kindly pointed us to the Velazco et al. and Jannis et al. papers which, to quote the reviewer, "explain the greater damage in plane wave illumination". In other words, these papers explain why the material damages faster in high resolution TEM than STEM. We were grateful to the reviewer for this as it helped us explain the difference in damage rate between TEM and STEM that we observe and which has also been well documented in the literature. This difference is why the previous best results on halide perovskites were obtained with conventional STEM rather than conventional TEM. It also most likely explains why there have been no conventional TEM images clearly resolving the edges published to date.

R2#03

> *"Line 102: "In contrast ptychography makes use of the direct beam, and provides much greater overall dose efficiency." - "much greater" is subjective, please drop the 'much' or quantify."*

We have changed the wording from 'much greater' to 'substantially greater'.

It seems quite clear that the ptychography provides substantially better contrast overall comparing the images from the ptychography and the ADF. In detail, the degree to which the

contrast improves depends on which type of column one examines, with the ptychography bringing greater advantages for the lighter columns than the heavier columns due to the atomic number contrast of ADF imaging. We summarise this as substantially greater *overall* efficiency, rather than going into greater detail in the main text. We do however discuss this at length in the SI.

R2#04

> *"Figure 2 caption: "Power spectra (cropped) of a and e" - Typographical error, this should read "a and d"."*

Thank you. We have fixed this.

R2#05

> *"Figure 2e) the identification of the 1.1 Angstrom reflection in the power spectrum is not convincing, this appears to be just noise in the power spectrum not a clear periodicity."*

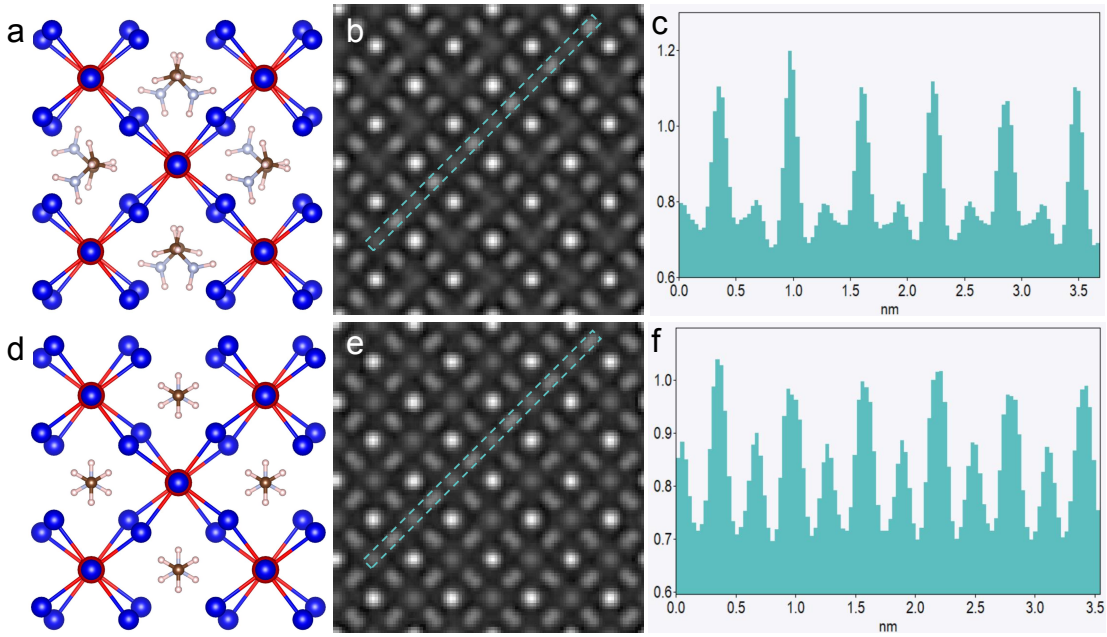
We agree that the *image of the power spectrum* we used in the figure did not show the higher frequency spots clearly enough, but this spot is above the noise level. The figure has been updated with a better image of the power spectrum using a better contrast level for viewing the higher frequency spots in the power spectrum and the insets. It now very clearly displays this periodicity.

R2#06

> *"line 108" The ptychography however is more efficient, and its significantly clearer images allow us to reveal the locations of all the atomic columns, including the MA+ molecules, as is also seen in simulations (SFigs. 25–28)." I am not fully convinced that the MA site is visible in figure 2(f). Furthermore in the simulations in figures S26 and S27, the MA molecules are not visible at doses equivalent to the experimental data shown in figure 2f (36e/A^2)."*

Significant bright molecular column shaped spots appear in exactly the correct locations for the MA columns in Fig. 2f. These spots do not appear in our simulations when the

MA columns are removed, and are therefore unlikely to be artifacts, so we do believe the image shows the MA columns. While the MA column we label as MA in Fig. 2f is split, this is consistent with the C and N atoms of the molecules aligning in an orientation where they form two atomic columns within the MA column, a possibility we already discuss and support with simulations in the SI section 'Imaging MA molecules in detail'.



RFig. 1. ePIE simulations illustrating the substantial MA column contrast difference resulting from different molecular orientations. (a) A structural model with a cross-aligned distribution of MA. (b) Ptychographic phase image simulated from the model in (a). (c) Line profile showing that the contrast of MA is significantly lower than that of I. (d) A structural model with all MA molecules aligned along the electron beam direction. (e) Ptychographic phase image simulated from the model in (d). (f) Line profile showing the substantially higher MA contrast compared to (c).

However, we agree that the contrast of the MA columns was significantly weaker in many of the simulations we presented than in the experiments, including the SI figures the reviewer refers to. This can be explained by the fact that the contrast of the MA columns is strongly dependent on their orientation as illustrated in the simulations shown in RFig. 1. For the majority of our simulations we assumed the MA molecules alternate in orientation in a cross-aligned fashion with a relatively low degree of alignment of the molecular atoms, resulting in a relatively low contrast for the MA columns as seen in the top half of RFig. 1. A much stronger molecular column contrast arises when the molecules are more aligned, as in the bottom half of RFig. 1. We believe the fact that we used a relatively low contrast

MA configuration in our simulations explains why they were showing a lower MA column contrast than the experimental images at the same dose. Especially with this explanation of the lower contrast in our original simulations, it seems clear that the MA sites are indeed visible in the experimental image in Fig. 2f.

We have now added RFig. 1 to the SI and point out there, in discussing the dose series simulations the reviewer refers to, that the contrast of the MA columns strongly depends on the orientations of MA molecules, and that this likely explains the stronger MA contrast in the experimental images.

R2#07

> *"Figure 3 - caption. Please add a description of the line scan in panel (b) to the caption."*

We have added this in the revision.

R2#08

> *"Figure 3 - caption "(d) and (f)..." Typographical error, this should read "(d) and (e)" there is no (f) in the figure."*

Thank you, we have corrected this in the revision.

R2#09

> *"Line 141 - "Region 1 exhibits both loss of material and formation of Pb crystallite (red box, Fig. 3e)." For the general reader please briefly explain how you come to the conclusion that a Pb crystallite has been formed (or refer the reader to supplementary information)."*

We have added this in the revision.

R2#10

> *"Line 162 - "Defects within the interior of the crystal can also strongly affect stability. Rather than transitioning to orthorhombic after 32 $e^{-}/Å^{2}$, this region damages much more rapidly and severely, appearing to lose significant material in an area around five*

times the initial defect size by 128 e^{-}/A^2." I believe you are referring to the images in Figure 4 panels a) - c) here please state this as it is not clear to the reader. Panel 4 c) is labelled 192.6 e/A^2 where as the text refers to 128 e/A^2. Is there a reason that the higher dose image is shown in the main text?"

Thank you for pointing out the issue here. The labelling was unfortunately incorrect. We have addressed this in our revision.

R2#11

"Figure 4 - The proposed structures of the 'stable' defects are shown. What is the proposed structure of the 'unstable' defect as imaged in panel a)?"

We have added a proposed structure for the Fig. 4a defect in SFig. 55e.

R2#12

"Line 178 – "The destabilising defect in Fig. 4a, contains a high proportion of I– vacancies producing an estimated net ionic charge of +2.5." I cannot find the proposed structure of the 'destabilising' defect in the main text or in the SI. As the relatively high 'net ionic charge' of this defect with respect to the two other identified defects is one of the conclusions of the work it is necessary to show the analysis leading to this conclusion. This can be added to the SI."

As noted in R2#11, we have added a proposed structure for the Fig. 4a defect in SFig. 55e.

R2#13

"Supplementary Information - "Estimating of defect charge states" I feel that there is insufficient information provided to be fully convinced by this analysis.

We have added significant additional information to the SI to the address this, including that requested by the referee below. We agree this is informative, however we also emphasise that our analysis is only intended as an estimate of the net charges and not an absolute quantification. Nonetheless, the variation in the values indicates a relatively small uncertainty.

*Specifically: * Over what distance about each atomic centre is the phase summed?*

Integration radii of 4 pixels (0.13 nm) were used for Pb-I columns. 3 pixel (0.10 nm) radii were used for the I and MA columns. We now note this in the SI.

** What is the variation of phase about nominally identical columns in the pristine region of the material? Can this be used to assign an uncertainty to the measurement?*

As we now discuss in detail the SI, we can compare the the variation in the phase of nominally identical atoms in pristine regions in both our simulated and experimental images. We use Atomap to extract the standard deviation σ of each column type. Our experimental standard deviations are in good overall agreement with the experimental values. Using the experimental standard deviations we now estimate uncertainties for the net charges of the defects shown in Fig. 4.

** What is the thickness limit over which the phase of, particularly the PbI column can be considered to be linear with thickness?*

As we now discuss in the SI, to assess the phase–thickness relationship we constructed a wedge-shaped $MAPbI_3$ model and simulated phase images to show that the phase is linear in our images up to the thickness of our sample, as long as we use the phase offset correction as we already do.

Without the phase offset, the 10,000 $e^-/Å^2$ phase profile reveals that contrast reversals occur on the Pb-I sites when the thickness exceeds approximately 2.8 nm. This means we cannot use the raw uncorrected data with the offset correction for reliable quantitative interpretation. However, after applying the phase offset with the value we optimised for our experimental thickness and used in all of our images unless otherwise noted, the Pb–I column exhibits a linear increase in phase up to our experimental thickness. Examining thicker regions than we used in our experiments, the phase starts to decrease at 4.7 nm as the phase offset used for our experimental thickness becomes increasing inappropriate. While we could explore alternative phase offsets for thicknesses above our those of our experiment, this is unnecessary to validate the analysis of our experimental data. Establishing linearity up to that of our experiments is already sufficient for our present study. The lighter I and MA columns maintain a linear phase–thickness relationship up to at least 6.6 nm, well

beyond our experimental thickness.

Low doses do not change the linearity, but do make the measurements noisy. Therefore we revised the third assumption we list in this section of the SI on estimating the defect charge states, from "the phase of each type of atomic site is proportional to the number of atoms they contain" to "Atomic columns exhibiting higher intensities are more likely to contain a greater number of atoms."

> ** The nanosheets have previously been determined to be approximately 3 unit cells thick. Does the quantification of phase here always give integer number of atoms? If not why not?"*

We do not count atoms in our quantification, but quantify the likely proportional changes in occupancy in the defects relative to the pristine regions. There is therefore no integer number of atoms to obtain.

We thank the referees for their constructive feedback and provide our point-by-point response below.

Referee #1 (Remarks to the Author):

This time, I have only a few technical issues that require the authors 'attention. However, I urge the authors to take the technical quality of the paper more seriously and conduct a thorough review to eliminate similar errors throughout the manuscript. I will recommend acceptance once these issues are properly addressed.

1. Dose notation consistency:
The dose notation must be consistent throughout the paper. Please correct the dose information in the following figures, and carefully review all other figures that contain dose values:
o Supplementary Figures 45c, 45f; 46c, 46d, 46e; and 50d.
(Note: This issue was already pointed out in the previous review—specifically, Supplementary Figure 46d in the earlier version.)

We believe this has now been fully corrected, including these Supplementary figures we missed last time.

2. Figure caption errors:
There are still several mistakes in the figure captions. For example:
o Figure 2: The phrase "... of a and e" should be corrected to "... of a and d"; the final reference to "f" should be "d".

This has been corrected.

3. Supplementary Document, Page 34 (last paragraph):
o The sentence: "...appear to be slightly closer to the I columns in the Pb-I columns in the next layer inside the material" is unclear and should be revised for clarity.

This has been corrected to "appear to be slightly closer to the I columns in the next layer inside the material". The next layer is a Pb-I and I row of columns, and we intended to refer to the I columns within it.

o Additionally, the two added pairs of lines in Supplementary Figure 39 should be explicitly mentioned and explained in the text.

We have now added the following explanatory text to the caption to SFig. 39, explicitly mentioning the pairs of lines and putting them in context:

"The first MA column in e can be seen to be slightly closer to the next layer inside the material in the image, and also in the MA-I line profile in g, with the first pair of columns being 3.0 Å apart and the next pair being 3.1 Å as indicated on the profile with black lines."

Referee #2 (Remarks to the Author):

In this second revision the authors have again made significant improvement to the manuscript adding substantially to the SI. All comments from my previous reviews have now been addressed either through changes and additions to the manuscript and SI or through robust rebuttal.

This work represents an impressive experimental acheivement, data analysis is comprehensive and conclusions well supported with extensive simulation. The manuscript is very well written and will be of interest to scientists studying metal-halide pervoskites and to the wider electron imaging community.

I found one additional typographical error in the supplimentary information, page 46:
" Therefore the phase of the Pb-I columns also begins to decrease above about 4.7 nm, but this does not matter **for the purposes of our present purposes** as our sample does not reach this thickness."

This has been corrected to: "Therefore the phase of the Pb-I columns also begins to decrease above about 4.7 nm, but this does not matter for our present purposes as our sample does not reach this thickness."