nature portfolio

# Peer Review File

# An X-ray-Emitting Proto-Cluster at $z\approx5.7$ Reveals Rapid Structure Growth

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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)
The paper presents the first evidence for the presence of extended X-ray signal in the direction of the protocluster candidated JADES-ID1 at z=5.68. The interpretation of the X-ray signature as thermal emission from forming intracluster gas poses a compelling challenge to the current paradigm of structure formation, which predicts that the first massive clusters should begin assembling at lower redshifts.

Despite the great potential of the results, I believe that the paper would benefit for more careful and detaild discussions of all the potential systematics and biases that could have affected the study. In particular, many statements in the manuscript are not supported by any reported quantitative tests, or are based on assumptions whose validity is not properly discussed. Overall, I think that these factors significantly hamper the overall robustness of the results, and I recommend a major revision before publication.

Major comments
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* Point sources *
In the main text, the authors state that they are using the dmfilth tool for inpainting the data in the direction of any point sources identified by wavdetect. Although statistically meaningful, such approach would necessarily introduce artifacts in the data. The authors should provide quantitative tests to show that such a procedure does not affect the results (for instance, the estimation of the likelihood map in Fig. 3).

Further, in the appendix, the authors mention that most of X-ray bright point sources fall outside the region where most of the extended X-ray emission is detected. In this regard, it would be important to support such a statement with a visual comparison of the distribution of point sources compares with respect to the overdensity members, along with the X-ray emission. As a more critical aspect, the authors claim that the only X-ray source falling in the inner 21" does not introduce any issue in the reported analysis. This is a statement that should be supported by a dedicated analysis, specifically aimed at quantifying potential contaminations from the point source to the extended emission.

Finally, X-ray studies of other protocluster complexes (e.g., Tozzi+2022, Vito+2024) have highglighted that protoclusters can exhibit enhanced X-ray AGN fractions compared to the field. As such, one could expect a significant emission from X-ray AGN within the overdense core of JADES-ID1, with an occupation fraction that might deviate from the background estimation. In this regard, the authors should discuss and present tests about the potential impact of X-ray sources that fall below the detection threshold of the wavdetect approach, in particular on the detection fidelity.

* Extended emission *

The detection and characterization of the extended X-ray emission present some inconsistencies and potential issues.

The identification in the smoothed image highlights a mild offset between the (smoothed) peak of the X-ray emission and the centroid of the galaxy overdensity. The authors are correct in stating that this could be due to dynamical processes and, in fact, is not a concerning issue. However, it is not clear how the smoothing operation could cause such a shift.
The main issue, however, is related to the follow-up analysis. The authors reconstruct radial profiles of the X-ray emission by fixing the center to the JWST centroid. Given the observed shift between the smoothed X-ray distribution and galaxy centroid, it is not clear how such a profile could provide a reliable representation of the radial properties of the X-ray emission.
Further, the peak in the likelihood map is found to be shifted both from the JWST and the X-ray centroids, but the authors do not provide any comments on this. Overall, this makes the intepretation of the detection and of the radial profile problematic.

Given the low-surface brightness nature of the detected emission, the authors should further test whether the observed radial profile does not result solely from the radial extraction itself or from residual contamination (see comment above).

Finally, the authors should explictly address the potential impact of low-surface brightness wings of the Chandra PSF on the profile reconstruction.

* Hardness ratio and X-ray bands *
The hardness ratio analysis provides a compelling argument in favor of the presence of extended emission. However, the whole result rely on the (rather key) assumption of the thermal nature of the X-ray signal. The authors should test whether other scenarios could reproduce the observed HR values.
Further, the authors should explicitly justify the specific choices of the energy bands used throughout the analysis and the impact on all the results. The 2keV limit would imply that the soft band used in the paper might still be contaminated by hard components, in turn introducing a spurious enhancement of the measured X-ray signal.

* Likelihood analysis *
The authors state that the likelihood map shows several high-significance peaks. However, only the one in the close proximity of the JWST centroid is considered meaningful, while the others are disregarded as due to fluctuations or low-z structures. A more detailed discussion should be provided on why such scenarios do not apply to main likelihood peak (e.g., whether the enhanced significance could be due to effect of line-of-sight interlopers or a chance association of the JWST centroid with noise features).

Regarding the generation of the likelihood map, how would the significance distribution change if the authors were to use a different centroid or different annuli?

* Multi-wavelength analysis *
In Champagne+2021, the X-ray emission originally assumed to be associated with thermal gas was found to be consistent with inverse Compton scattering off old radio galaxies within the protocluster core. This highlights the crucial role of a multi-wavelength approach. The authors should explore such possibility, and explicitly discuss whether any other measurements could support the reported results.
One important aspect would be to test whether the diffuse X-ray mission from JADES-ID1 has a detectable counterpart in the Sunyaev-Zeldovich (SZ) effect. The extreme mass reported by the authors would imply an SZ amplitude that could peak at values consistent with the core signal from massive, nearby systems, making the SZ signal potentially detectable even in wide-field surveys like ACT and SPT. The authors should directly address such a possibility, and discuss the implications of a potential detection or non-detection in the context of the reported results.

* Mass estimate *
The X-ray-based estimate of the mass of JADES-ID1 is the key ingredient for the main result of the paper, i.e., the early virialization of protocluster complexes. Such a crucial result would however benefit for a more accurate discussion of the many assumptions entering the determination of the mass itself.
First, a basic assumptions is that the X-ray emission is tracing virialized gas. This is however a rather strong assumption, as the bremmstrahlung can trace thermalised gas reservoirs independently of their virialization state.
Second, the authors are right about the limited appicability of the self-similar scaling relations in the context of protoclusters. However, they still base their conclusions on a luminosity-mass (and luminosity-temperature) scaling relation calibrated on local, high-mass systems (and which specific flavour of relation from Lovasari+2020 are they using?). As a simple test, the authors should check whether other scaling relations (potentially derived using different selection criteria) could impact their result.
Third, luminosity is derived with well specific assumptions about the plasma temperature and metallicity. Despite the temperature is consistent with the HR-based estiamate (although, see the comment about the thermal assumption), it is not obvious that the 0.3 metallicity value is appropriate for the system. The authors should at least discuss the impact of a different metallicity and tempearture on the mass estimate.

Fourth, several simulation works show that merger events are not only responsible of the injection of non-thermal pressure in the ICM, but also of transient, but significant boosts of the X-ray luminosity. The authors should discuss and estimate the potential impact of such a scenarion on their conclusions.

A final remark about the temperature. The authors derive a temperature of 2.18 keV using a luminosity-temperature scaling relation, with a luminosity computed by fixing the plasma temperature to 2keV. Isn't this a result of circular argument (with deviations resulting mostly from the intrisic scatter of the adopted scaling relation)?

Minor comments
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- Is there any specific reason for using the S/H ratio instead of the fractional difference (S-H)/(S+H)? Could the inherent skewness of the S/H distribution affect the results?

- The paper lacks references to more up to date works on high-redshift systems. For instance, the most recent catalogs by the ACT and SPT collaborations have an upper redshift limit between 1.6 and ~2. At the same time, in the past decade, targeted high-resolution observations with ALMA, MUSTANG2, and Chandra have been reported for a number of systems at higher redshift than the ones cited in the paper.

- The inferred surface brightnss profile is modelled using a beta model fixed beta. Despite this is a common approach, this could induce a bias in the estimation of the core radius, as a result of the unavoidable degeneracy between the two parameters. Although such a model is used only as a test of the extended nature of the X-ray emission, it might be worth exploring the impact of a more flexible model.

Referee #2

(Remarks to the Author)
In this paper, the authors present new Chandra X-ray observations of a JWST-detected proto-cluster candidate at redshift 5.7. An X-ray detection at such high redshift is in itself novel and of great interest to the community because it is not at all clear when the first X-ray clusters form. Moreover, we have no idea whether the usual low-redshift scaling relations between halo mass, stellar mass, and X-ray signal hold for the first clusters. Of course, a single detection does not allow for statistical analysis of a cluster sample, but it shows an avenue towards a new field of high-z X-ray clusters (or proto-clusters, as they are often called). The paper is clearly written and the figures mostly work. Thus, I feel that the paper is overall deserving of publication. However, I do see one major and a few minor issues that should be addressed if the paper is selected for publication.

1) A key aspect to put the paper into context is how likely or unlikely the formation of a cluster of the suggested mass is in an LCDM cosmology. The authors make some claims in this direction in the abstract and in other parts of the paper. However, I could not reproduce some of their halo-related calculations, leading to somewhat different conclusions.

1a) Both from the stellar and X-ray components, the authors infer a mass of about 1.3E13 Msun. They acknowledge that this is based on low-redshift scaling relations that may or may not hold, and that is fine. To test whether such a halo would be within LCDM expectations, they evaluate a commonly used mass function model at the redshift in question. Absent an estimate of the survey volume, the resulting likelihood of 3E-4 is hard to evaluate.

Given the survey footprint of 16x16'' and a survey range from z = 5-7, as stated in the paper, I derive an approximate survey volume of 1 physical Mpc^3 or 360 comoving Mpc^3. Integrating the mass function above the inferred cluster mass, I get a frequency of about 1E-12 halos per comoving Mpc^3, and thus an expected number of about 1E-9 halos of this mass in the survey volume. My interpretation of this number would be that finding the halo would essentially be impossible in LCDM. This is in contradiction to the conclusion that "structure may have formed more rapidly than predicted in some regions." Such variations are taken into account in the estimate. It is entirely possible that I made a mistake in my calculation, but it highlights the need for the paper to be clearer on how the statistical disagreement with LCDM was computed.

Another way to see how impossible this halo would be in LCDM is to compute its statistical significance or peak height, which is about 6 for the given mass and redshift (in a Planck cosmology). There are no such halos for all intents and purposes. This would mean that there are two interpretations for a detection, namely that either LCDM is totally wrong or the cluster mass is overestimated. The latter would not be surprising given that we do not know scaling relations at high redshift. I think it would be more interesting to ask what the most massive halo mass is that one could reasonably expect to find in the survey, and how far off known X-ray scaling relations this mass is.

1b) The authors produce an overdensity map to probe the statistical significance of their detection, namely via converting the galaxy overdensity into a significance. To do so, they "assume 30% cosmic variance," but this number is not justified. Cosmic variance is a sensible quantity only within a certain volume, and no such volume is given. If I understand the calculation correctly, the volume in question should be that of the overdensity, i.e. 250 physical kpc (using the diameter rather than the radius). This would be equivalent to 1-2 comoving Mpc, a scale on which the cosmic variance is a factor of a few rather than 30%. Again, it is absolutely possible that I missed something in this calculation.

1c) A halo mass of 1.3E13 at the given redshift implies a halo radius of R500c ~ 80 kpc. This would place the galaxies highlighted in Fig. 1 within 1-2 R500c, which makes sense (but it would be helpful to plot the estimated halo radius). The galaxy overdensity is measured within 125 kpc and 250 kpc, meaning about 3 R500c. This is fine, but should be stated for clarity.

2) A second difficulty I had while reading the paper were the repeated references to "virialization." The authors claim that an X-ray detection is a sign of virialization, but this seems far from obvious to me. Gas is typically heated to the "virial" temperature of halos in accretion shocks. Moreover, the distribution of galaxies shown in Fig. 1 looks fairly irregular. In summary, I am not sure what is meant by "virialization" and what exactly indicates that the cluster (assuming it is one) is virialized. Such a conclusion could be based on a smooth X-ray map, but that does not seem to be the argument the authors are making.

3) In Fig. 1, the translation between panels c and d is confusing because a lot of processing happens before the smoothing step. It would be good to show the processed but unsmoothed Chandra map as well. I was also a little confused about the white circles, since the text mentions 66 cluster candidates. The caption only mentions that they mark "likely" cluster members.

Referee #3

(Remarks to the Author)
The main result of the paper is the claimed detection of diffuse X-ray emission spatially associated with a high-redshift galaxy overdensity recently discovered with JWST. The authors interpret the diffuse X-ray signal as thermal emission from hot gas, and identify it as the forming ICM of the galaxy protocluster. This result, in principle, would be worth publication on Nature, as it would constitute the first detection of ongoing virialization of a massive halo at z=5.7, i.e. billions of years before the epoch at which most of the galaxy structures are expected to virialize, with a highly surprising and unexpected jump in redshift with respect to the most distant detection of ICM in the X-ray band to date (z=2.15).

However, major technical issues affect the analysis and results, as discussed below, and prevent us from recommending this paper for publication. The main point is that the detection significance of the X-ray diffuse emission and its eventual association with the high-redshift protoclusters are both highly questionable.

Diffuse X-ray emission detection.
This result is based on a relatively straighforward aperture photometry analysis. However, several steps of the analysis carry significant sources of uncertainty, especially in the background level estimate and removal.

- the background level is estimated in a region much larger than, and far away from, the region showing the potential diffuse emission. The Chandra background is low compared to other X-ray facilities, but it is still complex, especially in very deep fields as the CDF-S. Multiple components, both vignetted and unvignetted, concur in producing the total background, and some of them present spatial variations on scales of tens/hundreds of arcsecs. Assuming the average background computed on a large area, which is also spatially distant from the source region, and applying a simple correction based on the average exposure time in such an area does not account for the background complexity. Also, correcting by the exposure map may introduce significant biases, since the instrumental background is not vignetted and should not be corrected according to the exposure map. This issue is especially important for the detection of extended emission with low surface brightness, as that claimed in this paper, for which the background level is more than one order of magnitude larger than the excess signal possibly associated to the diffuse emission.

- As a consequence, the background level estimated from a large region may be affected by unnoticed bias. In addition, the uncertainty on the estimated background in the source region is underestimated, as it does not take into account that the smaller region is affected by stronger background fluctuations.

- The assessment of the detection significance should have taken into account that diffuse emission has been searched at multiple positions, corresponding to the (at least) six protoclusters presented in [8]. These counts as six independent experiments, increasing proportially the chance of detecting a positive fluctuation in at least one of them (see, e.g., the Bonferroni Correction method).

- The authors have checked for extended emission in the 3-7 keV, in addition to the 0.3-2 keV band. It remains unexplained why the 2-3 keV has been discarded. One explanation might be that the Chandra effective area varies strongly in that band, but the same is true for the 0.3-2 keV energy range.

- As a check, we performed an aperture photometry analysis similar to the one done in this manuscript, but in the 2-7 keV band. Assuming a more local background than that used in the draft, we found a positive fluctuation of about 400+-100 net counts. If confirmed, this would suggest a positive fluctuation in the instrumental background in that area (affecting both bands) rather than a signal from ICM emission. In fact, a hard detection at z~6 would be extremely hard to reconcile with thermal emission.

- The authors have not searched for emission at a specific position, but rather on an area "close enough" to the claimed protocluster center. Not many details are given on this point - e.g., what is the maximum distance between the diffuse X-ray emission and the protocluster center that the authors would accept to claim a physical association? The detection significance should take into account that positive fluctuations could have occurred over such an extended area. Instead, it is implicitly assumed that the authors looked at only one specific position and found the diffuse emission.

- Individual point sources are detected with the wavedetect tool and masked. The text report the detection of only one point source in the region of the diffuse emission, while dedicated source detection procedure (e.g., Luo+17) report the detection of at least two point sources at a few arcsec separation. While these are relatively faint sources, they can still contribute to the very faint diffuse emission. The paper does not report explicitly the position of the detected point source and the radius used to mask it, so further checks are not possible. Fig. 1 should have showed the X-ray image before and after the point source masking, and the regions used for masking, for clarity. Moreover, more details on the point-source masking/refill procedure whould be given. E.g., what statistics has been used during the masked region filling?

- The statistical error on the 144 net counts should be reported, as it already gives a sense of the detection significance. But see our comment on the severe underestimate of the uncertainties associated to the background subtraction.

- The position of the peak of the diffuse X-ray emission should be reported explicitly, to allow reproducibility.

- Fig. 2 presents the radial fit with a beta model of the claimed diffuse X-ray emission. More details should be given here to clarify the followed procedure, including the position of the radial center. In particular, it is unclear whether the smoothed or unsmoothed radial profile has been fitted. In the former case, the signal is artifically spread over nearby pixels, introducing a spatial correlation which is not present intrinsically. Any background fluctuation would have a similar radial profile. However, the most important issue here is that the authors fitted the background-subtracted signal (whether smoothed or not), and thus do not account for the background uncertainty. This is worrying, as the background counts are expected to be more than ten times higher than the signal counts.

- The method used to create Fig.3 and presented in Appendix C should be described in detail and more explicitly. In any case, Fig. 3 is used to support the detection of the diffuse emission close to JADES-01. The authors state that other positive peaks are visible in the figure, but at lower significance levels. For example, the signal at RA=3h32m35s and DEC=-27d46m30s appears to be comparable to, if not stronger than, the signal reported for JADES-01 when integrated over a suitably large region. Finally, following our previous note that any background fluctuation would have a similar radial profile when smoothed, we argue that combining the 0.3-2 keV excess with 3-7 keV non detection (but see our note that 2-7 keV images does have an even larger excess) and with the radially declining profile, would artificially enhance the significance of the detection. This cast doubts on the use of this map to visually represent the detection significance of the extended emission.

Association of the diffuse X-ray emission with the JADES-01 galaxy structure.
Under the assumption that the diffuse X-ray emission is real, its association with the actual galaxy structure at z=5.7 remains poorly justified.

- The method used to estimate the center of the galaxy structure is not described in this paper. The reference paper ([8]) is accepted but still unpublished, and the list of galaxies considered members of the structure is not reported in the online version. An unknown number of the galaxies are not confirmed spectroscopically. We assume that the structure center is simply the average position of its members. This estimate is clearly quite rough. Moreover, it does not account for the possible member galaxies that are located outside the JADES field of view, as similar structures at high redshift can extend to several arcmins (as also shown in Fig. 13 of [8]). Therefore, the position of the "center" of the galaxy structure is highly uncertain.

- The galaxy overdensity levels computed on different scales do not exceed 4sigma, which is a low value for a core of a massive galaxy structure (although it is a strong overdensity for being at z=5.7). The fact that the overdensity level does not increase significantly when computed on smaller scales again suggests that there is not a clear core position.

- Even assuming that the protocluster center used in this paper is the actual center, the spatial shift with the diffuse X-ray emission likely indicate a chance association, and thus a likely spurious (or foreground) nature of the X-ray emission. The authors address this point in the paper by refering to the young age and on-going virialization of the protocluster, but such an explanation is qualitative and speculative. We also stress again that the combined likelihood of the galaxy structure + diffuse X-ray emission has been computed assuming they are spatially coincident, while they are not.

- The fact that the center of the extended emission is not associated with a high density peak in the density distribution, is in strong disagreement with any reasonable expectation for proto-ICM at high redshift. Indeed, at much lower redshift (z~2) ICM emission is always associated with a beacon galaxy or a clear galaxy overdensity on small scale, in line with the expectation that the diffuse baryons are heated to >0.5 keV level in collapsing massive dark matter halos already associated to a significant stellar mass. Also, dynamical decoupling of diffuse baryons and galaxies is not expected at low overdensities (delta~4 for JADES-ID1) where the peculiar velocities are low.

Referee #4

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports.

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)
I would like to thank the authors for the detailed responses and for taking my comments into consideration. I think the revisions have significantly improved the manuscript, and have addressed the concerns I raised in my previous review.
I particularly appreciate the inclusion of the novel appendix that explores potential alternative origins of the observed X-ray emission.

Despite this, after reading the comments by Referee #3 and the respective responses by the authors, I would like to express a moderate concern about the interpretation of the likelihood map and any potential biases due to foreground structures.
The authors state that only the signal in the direction of JADES-ID1 "satisfies all of our X-ray criteria ... and is co-spatial with the independently identified JWST galaxy overdensity". Although this is clearly and effectively testing whether the signal could be statistically consistent with being associated with the z=5.7 complex, it is not testing against the presence of a foreground structure. In the rebuttal, the authors state that no low-redshift groups were identified in the JWST data, but how this was determined nor from where such an information was retrieved. For instance, the reference paper [8] does not provide a detail discussion on the level of contamination for each considered system.
Addressing such a point with a direct and quantitative test would be beneficial to the manuscript, and would significantly strengthen the reported finding.

And a minor comment: there might a typo in Eq. D1 (5" in the third term instead of 6")

Referee #2

(Remarks to the Author)
I appreciate the care taken by the authors to address my comments. I would be happy to recommend the paper for publication if a few smaller points below would be addressed. This is not meant as a statement about the more observational issues raised by the other referees.

1) Regarding the calculations:

1a) My apologies for the silly mistake, I did indeed use 16'' instead of 16'. With that fixed, my calculation of the likelihood of finding a halo of the given mass in a the survey volume gives a slightly lower value, namely 3E-5 for a 1E13 solar mass halo and 4E-7 for a 1.8E13 halo (the value quoted in the abstract). The exact values depend on the Hubble constant assumed and such, so the difference is not a big worry.

1b) I appreciate the explanation of the cosmic variance calculation. It makes sense that the CV in the UV luminosity function with the given magnitude limits is about 30%, but I wonder if this is the most salient question to ask in the present context? Another question is to what extent we should expect JADES to give an unbiased representation of the universe. For example, the likelihood of a 1E13 halo forming will drastically increase in regions with large-scale overdensity. The relevant quantity would be the variation of background density in volumes of JADES' size. This can be computed by integrating the non-dimensional variance based on the linear power spectrum.

2) Regarding the interpretation of the results:

2a) In line 247, the paper states "At these redshifts, the universe is predicted to host only a few dozen of systems with..." Do you mean the observable universe? The universe might be infinite.

2b) I also wonder if, in addition to the cosmic variance calculation, one might need to worry about the look-elsewhere effect with survey volumes as small as JADES. After all, the present paper would presumably not have been written had there not been a tentative detection. I realize that this effect is almost impossible to quantify, but it adds another caveat to a rigorous statistical interpretation.

2c) In line 258 and following, the authors connect the proto-cluster detection to the detection of "over-massive" galaxies in JWST. I think we should be careful with this analogy: so far, JWST galaxies can be explained by LCDM structure formation and very efficient star formation. While this also "challenges our understanding of early structure formation," it seems like it does not challenge LCDM itself but mostly our understanding of baryonic galaxy formation physics. In contrast, detecting hot gas from a 1E13 halo could presumably NOT be explained by some modification to baryonic physics, given that the physics of shock heating and gravity is relatively simple. I think it would be good to very briefly make this distinction.

Referee #3

(Remarks to the Author)
The authors have not addressed completely the two most relevant comments, namely 1) the significance of the diffuse soft-band X-ray emission and 2) the physical association with JADES-ID1, as reported below.

Detection significance:
According to the new information provided by the authors on the uncertainty of the net counts, the significance of the 0.3-2 keV detection is about 3sigma. This is the core of the results presented by the authors, before other ancillary information on the lack of hard-band emission and possible spatial distribution are accounted for. A 3-sigma detection can be considered a

good indication, but hardly a conclusive result. The derivation of the radial profile of such faint emission and the fit with a beta model are even more delicate issues.

One test that the authors can carry out to reinforce the detection claim is to split the observations into two sets with similar exposure times, and repeat their procedure. If the emission is real, they will detect it at about 2sigma in each set, otherwise a background fluctuation is more probable.

Physical association with JADES-ID1:
Even assuming that the diffuse emission is real, its physical association with JADES-ID1 remains highly uncertain. The authors refer to the fact that simulations predict irregular gas distribution. Using the same reasoning, the same simulations do not predict significant heating of such gas during the phases of infall at high redshift.

The authors also report several cases of unrelaxed and merging clusters in which the ICM emission is displaced from the center of the structures, but these structures are generally significantly larger and more massive than the z=5.7 structure, such that the gas can be heated via shocks more easily. In addition, these structures always show a well formed core centered on a beacon (radio, X-ray) and a more irregular morphology in the outer regions.

Explaining the shift between the X-ray emission and the optical center of JADES-ID1 with these examples is not fully justified.

Referee #4

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports.

Version 2:

Reviewer comments:

Referee #1

(Remarks to the Author)
I would like to thank the authors for the effort they put into updating the manuscript and, in particular, for performing the requested test of the potential foreground contamination. I find the results presented in the paper and in their report convincing.
Although the authors have satisfactorily addressed all the comments raised in the previous review rounds, I have a few minor points that should still be addressed.

- Given the comparison with the simulation works by Costa+2014 and Bennett+2024, presented as part of the response to Referee 3's comments, I do not think the statement that "cluster-scale virialization may have begun sooner than previously thought" is actually meaningful. First, the very concept of "virialization" (as already noted in earlier reviews and based on the broader discussion in the paper) may not hold, and the identification of X-ray emission is simply tracing thermalized gas. Second, if simulations indeed predict the presence of hot gas at temperatures >10^7 K, then the mere detection of X-ray gas cannot suggest a faster evolution than expected from current cosmological and physical models.

- I suggest avoiding references to JADES-ID1 as a potential progenitor of a Coma-like structure, as this reiterates the misconception of a direct link between massive overdensities at high redshift and massive clusters at z = 0. See, for instance, the discussion in Sect. 4.1 of Remus+2023 (https://ui.adsabs.harvard.edu/abs/2023ApJ...950..191R/abstract)

- One final test that I think should be added relates to the potential IC origin of the X-ray emission. The authors tested for the expected X-ray signal based on the (offset) radio source identified in the MeerKAT observations of the field. However, it might be beneficial to simulate the expected radio signal under the assumption that the entire X-ray emission originates from inverse Compton scattering from radio galaxies within the overdensity, and then check whether the predicted surface brightness would be detectable in the available radio observations.

- The authors included a reference to future X-ray and SZ telescopes and the possibility of using them to detect the signal from extended ICM haloes. However, this remains too vague. If specific forecasts exist in the literature, these should be explicitly cited. Otherwise, I recommend providing at least a simple order-of-magnitude estimate of the expected detectability in order to quantitatively support the statement.

Referee #2

(Remarks to the Author)
I thank the authors for their thoughtful modifications and responses to my comments. I am satisfied that all my comments have been addressed.

Referee #3

(Remarks to the Author)
The authors have addressed our comments on the previous paper draft, and the results of the additional tests we and the other referees proposed appear to go in right direction. We still consider both the detection significance and the potential physical association with the high-redshift overdensity as interesting but tentative, but we acknowledge that no other tests can further improve the significance of the claimed results or discard them.

Version 3:

Reviewer comments:

Referee #1

(Remarks to the Author)
I would like to thank the authors for addressing my comments and for the improvements made in the revised version of the manuscript. Despite this, I still have some concerns regarding the interpretation of the X-ray and radio data. Specifically, the authors derived constraints on the radio flux under the IC scenario, assuming a "typical" intracluster magnetic field of 4 µG. Such a value is rather extreme even for local galaxy clusters, which are observed to host magnetic fields with an average strength of ~2 µG up to z~1 (e.g., https://ui.adsabs.harvard.edu/abs/2021NatAs...5..268D). Given the strong dependence of the radio power on the assumed magnetic field (and the relevance of this test for the reported detection), it would be important to report how different assumptions about the intracluster magnetic field would affect the derived constraints. Further, to avoid appearing arbitrary, any specific quantitative assumption should be supported by appropriate references to the literature.

Finally, I would like to add a minor remark about the closing paragraph of the manuscript. I agree with the authors that a proper assessment of the high-z detection capability of future facilities is well beyond the scope of this paper. However, I would like to stress again the importance of providing the proper literature context for any such statements. The cited references are outdated and mention facilities that have undergone major rescoping or cancellations, undermining the authors' goal of "citing representative forecasts". Although I realize this is only a conclusive statement in the manuscript, I feel like more care should be taken – as the authors also note in their response, especially given the fast-evolving landscape of both X-ray (to name a few beyond AXIS and Lynx, mentioned by the authors: NewAthena, LEM, HUBS) and SZ/CMB (e.g., SPT-3G/3G+, CMB-HD, AtLAST, ALMA WSU) telescopes.

Version 4:

Reviewer comments:

Referee #1

(Remarks to the Author)
I would like to thank once again the authors for the thoughtful revisions and clarifications. I find that all my concerns and comments have been adequately addressed, and I consider the manuscript suitable for publication.

Referee #1 (Remarks to the Author):

The paper presents the first evidence for the presence of extended X-ray signal in the direction of the protocluster candidated JADES-ID1 at z=5.68. The interpretation of the X-ray signature as thermal emission from forming intracluster gas poses a compelling challenge to the current paradigm of structure formation, which predicts that the first massive clusters should begin assembling at lower redshifts.

Despite the great potential of the results, I believe that the paper would benefit for more careful and detaild discussions of all the potential systematics and biases that could have affected the study. In particular, many statements in the manuscript are not supported by any reported quantitative tests, or are based on assumptions whose validity is not properly discussed. Overall, I think that these factors significantly hamper the overall robustness of the results, and I recommend a major revision before publication.

Major comments

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* Point sources *

In the main text, the authors state that they are using the dmfilth tool for inpainting the data in the direction of any point sources identified by wavdetect. Although statistically meaningful, such approach would necessarily introduce artifacts in the data. The authors should provide quantitative tests to show that such a procedure does not affect the results (for instance, the estimation of the likelihood map in Fig. 3).

The Chandra images shown in Fig. 1(d) and (e) were produced for qualitative illustration only. After detecting point sources with wavdetect, we used dmfilth to fill each source region with counts taken from the local background level, and then applied a Gaussian smooth to highlight large-scale diffuse emission. This highly processed image with the point source location filled was NOT used in any of our quantitative analysis. We explicitly mention this in the revised manuscript.

All measurements, namely the surface-brightness profiles, hardness ratios, and the combined likelihood map in Fig. 3, were carried out using the unsmoothed, point source-excluded Chandra images (with excluded areas omitted and not refilled). Therefore, there are no artifacts present from the inpainting or smoothing of the images in our derived profiles, detection significance, or the inferred physical properties of the JADES-ID1 proto-cluster. Furthermore, because the re-filled regions in Fig. 1(d) and (e) are taken directly from local background regions, even these display panels introduce no statistical bias into our results.

Further, in the appendix, the authors mention that most of X-ray bright point sources fall outside the region where most of the extended X-ray emission is detected. In this regard, it would be important to support such a statement with a visual comparison of the distribution of point sources compares with respect to the overdensity members, along with the X-ray emission. As a more critical aspect, the authors claim that the only X-ray source falling in the inner 21" does not introduce any issue in the reported analysis. This is a statement that should be supported by a dedicated analysis, specifically aimed at quantifying potential contaminations from the point source to the extended emission.

We have added a new figure to Appendix A showing the merged 0.3-2 keV Chandra image both before and after excluding point sources. The locations of the candidate cluster member galaxies are also overlaid on these images. This direct comparison demonstrates that none of the galaxies are co-spatial with a bright X-ray source. These images also illustrate that our large point source exclusion regions effectively prevent spillover counts from the detected X-ray sources, hence these point sources do not contaminate the analysis of the diffuse emission.

The spillover counts from resolved point sources do not bias our measurement of diffuse emission. As shown in the new Figure A1, our exclusion regions around each point source are large enough to enclose virtually all of their flux. For example, the brightest point source within the 21 arcsec source aperture has an elliptical exclusion region with minor and major axis radii of 2.3 arcsec and 2.8 arcsec, respectively. Given the off-axis angle of this source and this large source region, the Chandra PSF encloses ~96% of the source counts. Since this source has 115 net counts in the soft X-ray band (Luo et al. 2017, ApjS, 228, 2), no more than ~5 photons can contribute to the diffuse emission. In the revised manuscript, we also exclude two fainter sources (see our response to Referee #3's question below), with each source having ~10 counts in the soft band. Since the source exclusion region includes ~96% of the source counts, we estimate that no more than one count contributes to the diffuse emission in total from these two sources combined. It is important to realize that cosmic X-ray background sources are not only present in the source but also in the background region. Therefore, spillover counts from many point sources contribute to the local background and raise the overall background level by approximately the same amount. In other words, spillover counts from point sources add an approximately equal contribution to both the source and background apertures, and hence cancels out when subtracting the background. Taken together, these facts demonstrate that spillover counts from point sources do not significantly affect our measurement of the extended emission. We have revised the manuscript and we provide details on the question of spillover counts in Appendix A.

Finally, X-ray studies of other protocluster complexes (e.g., Tozzi+2022, Vito+2024) have highglighted that protoclusters can exhibit enhanced X-ray AGN fractions compared to the field. As such, one could expect a significant emission from X-ray AGN within the overdense core of JADES-ID1, with an occupation fraction that might deviate from the background estimation. In this regard, the authors should discuss and present tests about the potential impact of X-ray sources that fall below the detection threshold of the wavdetect approach, in particular on the detection fidelity.

The referee is correct that lower-redshift clusters often show an elevated AGN fraction. However, in the case of JADES-ID1 we found no evidence for significant AGN contamination. We inspected the positions of all candidate member galaxies and none of them were co-spatial with a detected X-ray point source (see our new Fig. A1). As noted in the paper, "none of the high-redshift galaxies host a luminous X-ray AGN." Recent X-ray studies of JWST-selected galaxies at z>5 consistently failed to detect X-ray emission from individual galaxies and even from stacked galaxy samples (Ananna et al. 2024; Yue et al. 2024; Kocevski et al. 2024; Sacchi et al. 2025). This X-ray faintness may imply that high-redshift AGN are intrinsically X-ray weak, heavily obscured, or they could have very low duty cycles. The non-detection of X-ray emission from the high-redshift galaxies is consistent with this picture.

As an additional check, we co-added (i.e. stacked) the Chandra counts at the positions of the candidate member galaxies within our source aperture in the 0.3-2 keV and 3-7 keV bands. To this end, we used a 1" source aperture and a 3"-6" local background annulus. The stacks show no statistically significant emission. Specifically, we detected -0.2 +/- 8.2 counts in the soft and -10.9 +/- 9.2 counts in the hard band. Thus, this analysis confirms that unresolved AGN do not contribute to the diffuse signal. We added this information to the manuscript: we provide a brief explanation in the main body and a more detailed discussion in the new Appendix C.

We also emphasize that the large-scale diffuse emission is only detected in the soft band (0.3-2 keV) and is absent in the hard band. A population of obscured AGN would instead produce a harder spectrum, which would also result in a detection in the hard band. Finally, we note that both the surface brightness profile and spectral hardness ratio of the emission match a few-keV thermal plasma rather than a population of AGN. Together, these facts demonstrate that undetected AGN cannot account for the extended X-ray emission in JADES-ID1.

* Extended emission *
The detection and characterization of the extended X-ray emission present some inconsistencies and potential issues.

The identification in the smoothed image highlights a mild offset between the (smoothed) peak of the X-ray emission and the centroid of the galaxy overdensity. The authors are correct in stating that this could be due to dynamical processes and, in fact, is not a concerning issue. However, it is not clear how the smoothing operation could cause such a shift.
The main issue, however, is related to the follow-up analysis. The authors reconstruct radial profiles of the X-ray emission by fixing the center to the JWST centroid. Given the observed shift between the smoothed X-ray distribution and galaxy centroid, it is not clear how such a profile could provide a reliable representation of the radial properties of the X-ray emission.
Further, the peak in the likelihood map is found to be shifted both from the JWST and the X-ray centroids, but the authors do not provide any comments on this. Overall, this makes the intepretation of the detection and of the radial profile problematic.

In the original manuscript, we did not mean to imply that the applied 15 pixel (~7.5 arcsec) Gaussian smoothing fully causes the offset between the X-ray centroid and the JWST overdensity peak. Rather, the smoothing and binning limits our ability to determine the X-ray centroid with a few arcseconds precision. That said, we agree with the referee that the ~8 arcsec offset between the galaxy overdensity and the hot gas is not a concern. As we noted in our response to Referee #3, such offsets are common in dynamically active or merging systems. In a young proto-cluster, such as JADES-ID1, sub-cluster infall, sloshing, or mergers can displace the ICM peak from the galaxy distribution by similar or even larger amounts. This is supported by a wide range of observational and theoretical studies, which we present below (see our response to Referee #3). We have also revised the text to clarify the observed X-ray/optical offset in low- and high-redshift clusters and we also noted the centroid uncertainty introduced by the smoothing.

We agree with the reviewer that centering the profile on the JWST-derived position may not fully capture the X-ray surface-brightness distribution. Therefore, following the referee's comment, we have re-computed the radial profile presented in Fig. 2 using the X-ray-derived centroid and replaced Fig. 2 accordingly. When comparing our original profile using the JWST centroid with the revised profile, we find that neither the overall shape of the profile nor the best-fit beta-model parameters (namely the core radius) change in any significant way. This is due to the fact that our radial bins are 5-10 arcsec, so the offset between the X-ray and JWST centroids moves only a small fraction of the counts between adjacent rings. We have updated the manuscript with the new profile, revised the corresponding text, and values.

We thank the referee for pointing out the apparent offset between the significance map peak on Fig. 3 and the X-ray emission centroid on Fig. 1e. In fact, the panels in Fig. 1 were rotated by about 30° purely for visual clarity, whereas the original significance map in Fig. 3 was shown without that same rotation, which resulted in the apparent (but not real) difference in centroids. We have now updated Fig. 3 and applied the same rotation, hence it shares the same orientation as the panels in Fig. 1. This now demonstrates that the highest significance region is, in fact, co-spatial with the smoothed X-ray emission. We also verified this by comparing the centroids derived from the smoothed map in Fig. 1 and the significance map in Fig. 3, and found that they agree to within 2 arcsec.

Given the low-surface brightness nature of the detected emission, the authors should further test whether the observed radial profile does not result solely from the radial extraction itself or from residual contamination (see comment above).

As we discussed above, we have carried out several independent checks to verify that the declining surface brightness profile is not an artifact of our extraction or residual background fluctuations. First, the surface brightness profiles were constructed based on the unsmoothed, point source-masked 0.3-2 keV band image. Therefore, neither the smoothing nor the inpainting of the excised source region has introduced any spurious effects. Second, we improved our background subtraction approach and point source masking method following suggestions from Referee #3. After adapting the slightly revised analysis, we still see the same extended signal associated with JADES-ID1. Finally, given the sharp PSF of Chandra (~90% of the encircled energy is contained within 1 arcsec at the position of JADED-ID1), scattering of photons between the 5-10 arcsec wide annuli in our surface brightness profile is negligible. Together, these tests demonstrate that the radial profile reflects genuine, extended ICM emission rather than an artifact of our masking or background subtraction procedures.

Finally, the authors should explictly address the potential impact of low-surface brightness wings of the Chandra PSF on the profile reconstruction.

Because the JADES-ID1 proto-cluster lies about 1 arcmin from the aimpoint of the ACIS-I observations, the Chandra PSF is very good at this position. Indeed, based on the PSF maps of the observations, we find that about 90% of the encircled energy falls within 1 arcsecond at 1.5 keV energy. Our radial profile uses three broad annuli with 5-10 arcsec width, so any photons scattered into the PSF wings would only redistribute counts within the same bins and cannot produce the steadily declining profile we observe. We therefore conclude that PSF-wing contamination is negligible and does not bias our measurement.

* Hardness ratio and X-ray bands *
The hardness ratio analysis provides a compelling argument in favor of the presence of extended emission. However, the whole result rely on the (rather key) assumption of the thermal nature of the X-ray signal. The authors should test whether other scenarios could reproduce the observed HR values.
Further, the authors should explicitly justify the specific choices of the energy bands used throughout the analysis and the impact on all the results. The 2keV limit would imply that the soft band used in the paper might still be contaminated by hard components, in turn introducing a spurious enhancement of the measured X-ray signal.

We agree that, in principle, any non-thermal or unresolved point-source component could affect the hardness ratio. However, the combination of soft band detection and hard band non-detection already excludes all other viable explanations for the observed soft excess. Specifically, all resolved point sources were excised before analyzing the diffuse emission, and tests of spillover counts from masked sources show that they do not contribute significantly (see our detailed response above). Our stacking analysis of the candidate member galaxies within the 21 arcsec source aperture yields no significant counts in either the soft or hard bands, which rules out that a population of faint, unresolved AGN are responsible for the observed signal. We also exclude inverse-Compton emission, which would produce a substantial hard-band signal (see our response below). Thus, our hardness ratios are consistent with a few-keV temperature thermal plasma. We discuss these points in the new Appendix C, where we exclude these alternative scenarios. We also note that in our manuscript we primarily use the hardness-ratio analysis to characterize the ICM temperature.

Using the 0.3-2 keV energy range as our soft band is standard practice when studying diffuse X-ray emission, and it also maximizes sensitivity to few-keV plasmas at high redshift while avoiding the strong instrumental features around 2.1 keV. In particular, we extend the lower bound down to 0.3 keV because the early Chandra observations of CDFS at these energies are relatively free of molecular contamination, yielding better soft band sensitivity. Even if the ICM were heated above 2 keV by early mergers or shocks at z ~ 5.7, most of the observed counts would still fall below 2 keV but above 1.2 keV. Indeed, our measured hardness ratios show that the bulk of the counts lie in 0.3-1.2 keV, consistent with a thermal ICM. Our hard band is 3-7 keV, which entirely avoids the variable Au fluorescence complex (see our detailed response to Referee #3), while it remains sensitive to genuine hard emission from unresolved AGN. We note that all detected point sources were excised before measuring the diffuse emission associated with JADES-ID1, so any residual hard band flux would indicate non-thermal contamination. In practice, the 0.3-2 keV counts trace the few-keV ICM, and the non-detection in the 3-7 keV band confirms that a significantly harder component is not present. We have updated Appendix A with a more detailed justification of the energy range.

* Likelihood analysis *
The authors state that the likelihood map shows several high-significance peaks. However, only the one in the close proximity of the JWST centroid is considered meaningful, while the others are disregarded as due to fluctuations or low-z structures. A more detailed discussion should be provided on why such scenarios do not apply to main likelihood peak (e.g., whether the enhanced significance could be due to effect of line-of-sight interlopers or a chance association of the JWST centroid with noise features).

The likelihood map presented in Fig. 3 was not produced as a tool for identifying X-ray clusters in a blind search. In other words, we were not looking for random X-ray peaks but rather asking whether significant diffuse X-ray emission appears where JWST tells us a proto-cluster should be. The fact that the highest significance feature in the likelihood map is co-spatial with the JWST centroid strongly argues against a chance coincidence. If the main peak were simply a background fluctuation or a low-redshift interloper, we would not expect it to coincide so closely with a pre-selected galaxy overdensity at z ~ 5.7. We also note that none of the other peaks on this map fall near high-redshift galaxy overdensities, so these are most likely lower-z structures or random fluctuations. Taken together, the spatial agreement between the JWST-defined centroid and our highest significance X-ray feature makes it extremely unlikely that the detection is due to an unrelated line-of-sight fluctuation or interloper. We have revised the last paragraph of Appendix D to provide more explanation on the likelihood map.

Regarding the generation of the likelihood map, how would the significance distribution change if the authors were to use a different centroid or different annuli?

Each pixel location in the likelihood map is the centroid for the circles/annuli used to extract the counts from regions, which are then used to calculate the combined probability described in Appendix D. Since the pixel size of the likelihood map is ~2.5” (i.e. Chandra pixels are binned by 5) and the smallest extraction region is 6”, the pixels are not statistically independent due to the oversampling. However, each pixel value reflects the probability of (1) significant counts above background within 6”; (2) a rising profile from 6-10” annulus and 0-6” annulus, and (3) a non-detection in the hard band within 6”. We note that varying the size of the extraction regions does not change the map qualitatively. The particular choice of the 0-6” and 6-10” radii reflects the equal division of source counts over the two bins for an assumed beta model with beta=0.6

and $r_c$=7.1" (as measured from the profile in Fig. 2). We have revised and improved the description of the significance map in Appendix D.

* Multi-wavelength analysis *
In Champagne+2021, the X-ray emission originally assumed to be associated with thermal gas was found to be consistent with inverse Compton scattering off old radio galaxies within the protocluster core. This highlights the crucial role of a multi-wavelength approach. The authors should explore such possibility, and explicitly discuss whether any other measurements could support the reported results.
One important aspect would be to test whether the diffuse X-ray mission from JADES-ID1 has a detectable counterpart in the Sunyaev-Zeldovich (SZ) effect. The extreme mass reported by the authors would imply an SZ amplitude that could peak at values consistent with the core signal from massive, nearby systems, making the SZ signal potentially detectable even in wide-field surveys like ACT and SPT. The authors should directly address such a possibility, and discuss the implications of a potential detection or non-detection in the context of the reported results.

We thank the referee for highlighting the inverse-Compton interpretation of Champagne et al. (2021). To test this scenario, we inspected the MeerKAT 1.28 GHz map of the CDFS region (Hale et al. 2025, MNRAS, 536, 2187). We identified a weak radio point source within our 21 arcsec aperture with flux density of 13.7 +/- 1.4 $\mu$Jy. However, this radio source is co-spatial with a galaxy at z=1.173 (CANDELS J033230.91-274649.5) and one of the faint (excluded) X-ray point sources, implying that the radio source is not associated with the z~5.7 proto-cluster, JADES-ID1. Moreover, this radio/X-ray source lies ~11.5 arcsec away from the peak of the diffuse X-ray emission, further ruling out an association.

Nevertheless, we proceeded to predict the IC contribution that such a radio-bright lobe would produce if it was responsible for our soft-band detection at z=5.7. From the measured 0.3-2 keV luminosity of the diffuse emission, we infer a magnetic field of 2.5 $\mu$G. However, in this case, the same electron population would yield ~230 net counts in the 3-7 keV band luminosity, which would correspond to a >4 sigma detection. Such a hard band detection is strongly inconsistent with the data, ruling out this scenario. Since this radio source is actually associated with a z=1.173 galaxy we can calculate the predicted X-ray flux using a typical magnetic field of 1 $\mu$G. In this case, we predict an IC X-ray flux that is an order of magnitude fainter than the observed X-ray luminosity. We also note that one of the faint and excluded X-ray sources in our aperture is associated with the z=1.173 galaxy and the radio source. This X-ray point source has ~10 net counts, which makes it broadly consistent with such IC predictions. However, the X-ray photons associated with this source have been excised from the analysis, so they do not contribute to the extended emission associated with JADES-ID1.

Taken together, (1) the only radio emitter in our aperture is at z=1.173 and is offset from the X-ray peak; (2) the predicted hard band IC counts substantially exceed our X-ray non-detection; (3) at z=1.173 we expect an order of magnitude lower X-ray flux from IC than observed. Thus, the large-scale diffuse emission cannot originate from IC emission and we conclude that the diffuse soft X-ray emission arises from thermal ICM associated with the z~5.7 proto-cluster. We added a discussion about the above points to the new Appendix C.

The JADES/Chandra Deep Field South Field is not covered by either ACT or the SPT surveys. Indeed, the SPT survey covers 2800 deg^2 but it does not include JADES/CDFS (Bleem et al. 2020, ApJS, 247, 25). Similarly, the ACT survey also does not cover the JADES/CDFS field (Hilton et al. 2021, ApJS, 253, 3). But even if JADES-ID1 was covered, neither SPT nor ACT could yield a significant detection of the JADES-ID1 proto-cluster. Using the scaling relation of Arnaud et al. 2010 (A&A, 517, 92A) a proto-cluster with 1e13 Msun halo mass at z=5.7 is expected to produce an SZ signal on the order of Y_500 ~ 1e-7 arcmin^2. However, the typical noise in these maps is at least an order magnitude higher (~1e-6), see Bleem et al. 2020, (ApJS, 247,25) and Hilton et al 2021 (ApJS, 253, 3). In other words, the expected signal would lie well below their detection threshold.

This conclusion is supported by the observed sample of galaxy clusters by these SZ surveys. Namely, SPT establishes a flux-limited sample of clusters with completeness of ~3e14 $M_{sun}$ mass at z<1.8 (Bleem et al. 2020, ApJS, 247, 25). Similarly, ACT detected clusters above 4e14 $M_{sun}$ with redshifts <1.7. Thus, the lower limit of cluster masses detected by these SZ surveys are an order of magnitude above the mass of JADES-ID1. While Planck does have coverage of the JADES field, due to its sensitivity and beam size it also does not have the sensitivity to detect a JADES-ID1-like proto-cluster. In the revised manuscript we note that current generation SZ experiments lack the sensitivity to detect JADES-ID1-like systems at z=5.7.

* Mass estimate *
The X-ray-based estimate of the mass of JADES-ID1 is the key ingredient for the main result of the paper, i.e., the early virialization of protocluster complexes. Such a crucial result would however benefit for a more accurate discussion of the many assumptions entering the determination of the mass itself.
First, a basic assumptions is that the X-ray emission is tracing virialized gas. This is however a rather strong assumption, as the bremmstrahlung can trace thermalised gas reservoirs independently of their virialization state.

We agree with the referee (which also echoes a similar point raised by Referee #2) that our detection of soft X-ray emission, consistent with bremsstrahlung, demonstrates shock-heating to few-keV temperatures, not full hydrostatic equilibrium. We have therefore replaced the term "virialized" with "onset of ICM heating" throughout the revised paper. In the revised manuscript we now stress that the detection of extended, few-keV thermal emission at z ~ 5.7 shows that the ICM has already been heated on cluster scales even though JADES-ID1 is unlikely to be in a fully virialized state.

Second, the authors are right about the limited appicability of the self-similar scaling relations in the context of protoclusters. However, they still base their conclusions on a luminosity-mass (and luminosity-temperature) scaling relation calibrated on local, high-mass systems (and which specific flavour of relation from Lovasari+2020 are they using?). As a simple test, the authors should check whether other scaling relations (potentially derived using different selection criteria) could impact their result.

In the revised manuscript, we specify that we use the "all clusters" L-M and M-T relations from Lovisari et al. 2020 (ApJ, 892, 102). For the specific values, please see their Table 2. However, we adopt a self-similar evolution parameter, namely gamma = -1 for M-T and gamma = 7/3 for L-M, see their Table 1. Following the referee's suggestion, we also tested the alternative L-M and M-T relations derived from the subset of "disturbed clusters" and "relaxed clusters" from the same work. In all cases, the resulting kT and $M_{500}$ values remain consistent with our quoted values to well within 1 sigma. Moreover, Lovisari et al. (2020) also compare their results with other frequently used scaling relations from the literature. Although the normalizations and slopes of those relations differ slightly between various studies, they agree with Lovisari et al. "all clusters" fit within their respective 1 sigma uncertainties over the relevant mass range. In the revised manuscript, we corrected a small mistake in our scripts that converted X-ray counts to flux and then to the corresponding kT and $M_{500}$ values. While these changes are small and do not affect our conclusions, we have updated the updated values. We also now report all quantities to one significant digit.

Third, luminosity is derived with well specific assumptions about the plasma temperature and metallicity. Despite the temperature is consistent with the HR-based estiamate (although, see the comment about the thermal assumption), it is not obvious that the 0.3 metallicity value is appropriate for the system. The authors should at least discuss the impact of a different metallicity and tempearture on the mass estimate.

We agree with the referee that the 0.3 Solar metallicity is an assumption and not a direct measurement. However, observational studies of low-redshift and moderate-redshift clusters (McDonald et al. 2016, 826, 124) do not reveal any significant evolution in the ICM metallicity out to z ~ 1.5. Moreover, recent simulations of very high-redshift proto-clusters (Bennett et al. 2024, MNRAS, 527, 1033) suggest a metallicity in the range of 0.2-0.5 Solar at z = 6. Therefore, adopting the 0.3 Solar metallicity is a reasonable approach. To test the sensitivity of our luminosity estimate, we recalculated the X-ray luminosity over the range of 0.1-0.5 Solar metallicity, while keeping all other parameters the same. In that exercise, L_X changed by <5%. We conclude that even if the true metallicity of JADES-ID1 at z ~ 5.7 differs from the assumed 0.3 Solar value, our main results remain unaffected. Similarly, variations in the best-fit temperature within a plausible range will not significantly alter the inferred total mass of the proto-cluster. We have added these references to the main body of the paper to justify our choice of metallicity and we also note the negligible impact on the inferred mass when the metallicity or temperature is varied within this plausible range.

Fourth, several simulation works show that merger events are not only responsible of the injection of non-thermal pressure in the ICM, but also of transient, but significant boosts of the X-ray luminosity. The authors should discuss and estimate the potential impact of such a scenarion on their conclusions.

We agree that major mergers can temporarily boost both the X-ray luminosity and temperature of a halo, as demonstrated by previous studies (Ricker & Sarazin 2001, ApJ, 561, 621; Ritchie & Thomas 2002, MNRAS, 329, 675). However, several simulations show that these transient boosts introduce only a modest bias in the luminosity-mass or temperature-mass scaling relations (Ritchie & Thomas 2002, MNRAS, 329, 675; O'Hara, et al. 2006, ApJ, 639, 64; Rasia et al. 2011, ApJ, 729, 45). While this would imply a small decrease in our inferred halo mass, JADES-ID1 would nevertheless remain an extremely massive system at z=5.7. We also note that our calculations about the likelihood of detecting a massive halo at z=5.7 were done with cluster masses of 1e13 Msun and 2e13 Msun, thus fully encompassing this uncertainty. We expanded our previous discussion in the original draft to clarify  that mergers can boost the ICM temperature, we expanded that discussion to clarify the effects on the X-ray luminosity and their modest impact on our mass estimates.

A final remark about the temperature. The authors derive a temperature of 2.18 keV using a luminosity-temperature scaling relation, with a luminosity computed by fixing the plasma temperature to 2keV. Isn't this a result of circular argument (with deviations resulting mostly from the intrisic scatter of the adopted scaling relation)?

The referee is correct that we adopted a 2 keV thermal plasma model to generate our exposure maps, which are in turn used to derive the X-ray luminosity and total mass. However, this does not introduce a circular argument, because the exposure map normalization is only weakly sensitive to the assumed temperature of the thermal plasma model (APEC). To demonstrate this, we produced exposure maps with kT = 1, 2, 3, and 4 keV temperatures and measured their average values at the position of JADES-ID1. Relative to the kT=2 keV exposure map values, the resulting normalizations differ by less than +/- 10% across this temperature range. This implies that our luminosity calculations would also carry a less than +/- 10% change if we were to use any of these other temperatures for the exposure map generation. In other words, our choice of using a 2 keV model does not force the result but we would infer the same ICM temperature even if we used any other reasonable plasma temperature for our exposure maps.

Minor comments
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- Is there any specific reason for using the S/H ratio instead of the fractional difference (S-H)/(S+H)? Could the inherent skewness of the S/H distribution affect the results?

We tested both definitions of hardness ratios, and found that the (S-H)/(S+H) ratio yields broader credible temperature intervals than the applied S/H ratio. In both cases, we use BEHR to compute the full posterior for the counts in each energy range, implying that any skewness in the underlying distributions is captured in the uncertainties. Nevertheless, our main conclusion is independent of which hardness ratio we adopt. Specifically, we conclude that the observed emission is consistent with thermal emission.

- The paper lacks references to more up to date works on high-redshift systems. For instance, the most recent catalogs by the ACT and SPT collaborations have an upper redshift limit between 1.6 and ~2. At the same time, in the past decade, targeted high-resolution observations with ALMA, MUSTANG2, and Chandra have been reported for a number of systems at higher redshift than the ones cited in the paper.

We have updated the references in the first paragraph of the manuscript and included more recent studies to acknowledge some of the more recent high-redshift cluster observations.

- The inferred surface brightnss profile is modelled using a beta model fixed beta. Despite this is a common appoach, this could induce a bias in the estimation of the core radius, as a result of the unavoidable degeneracy between the two parameters. Although such a model is used only as a test of the extended nature of the X-ray emission, it might be worth exploring the impact of a more flexible model.

We agree that fitting a beta-model with both the beta parameter and core radius as free parameters can result in degeneracies, especially with limited signal-to-noise data. To avoid such degeneracy, we fixed beta = 0.6 and fit only for the core radius. We note that the selected core radius is the canonical value measured in many low- and intermediate-redshift clusters (e.g. Vikhlinin et al. 2006, ApJ, 640, 691). We thus obtained a core radius of 7.1" ± 3.9", where the uncertainties are driven by the relatively low number of counts in the annuli. While it would be ideal to adopt a more flexible functional form, such as the double-beta model by Vikhlinin et al. 2006, our limited surface-brightness profile does not have enough degrees of freedom to constrain multiple beta-model components. Therefore, our beta = 0.6 assumption is not intended as a precise measurement of the core radius, but as a test of the truly extended nature of the CGM.

Referee #2 (Remarks to the Author):

In this paper, the authors present new Chandra X-ray observations of a JWST-detected proto-cluster candidate at redshift 5.7. An X-ray detection at such high redshift is in itself novel and of great interest to the community because it is not at all clear when the first X-ray clusters form. Moreover, we have no idea whether the usual low-redshift scaling relations between halo mass, stellar mass, and X-ray signal hold for the first clusters. Of course, a single detection does not allow for statistical analysis of a cluster sample, but it shows an avenue towards a new field of high-z X-ray clusters (or proto-clusters, as they are often called). The paper is clearly written and the figures mostly work. Thus, I feel that the paper is overall deserving of publication. However, I do see one major and a few minor issues that should be addressed if the paper is selected for publication.

1) A key aspect to put the paper into context is how likely or unlikely the formation of a cluster of the suggested mass is in an LCDM cosmology. The authors make some claims in this direction in the abstract and in other parts of the paper. However, I could not reproduce some of their halo-related calculations, leading to somewhat different conclusions.

1a) Both from the stellar and X-ray components, the authors infer a mass of about 1.3E13 Msun. They acknowledge that this is based on low-redshift scaling relations that may or may not hold, and that is fine. To test whether such a halo would be within LCDM expectations, they evaluate a commonly used mass function model at the redshift in question. Absent an estimate of the survey volume, the resulting likelihood of 3E-4 is hard to evaluate.

Given the survey footprint of 16x16'' and a survey range from z = 5-7, as stated in the paper, I derive an approximate survey volume of 1 physical Mpc^3 or 360 comoving Mpc^3. Integrating the mass function above the inferred cluster mass, I get a frequency of about 1E-12 halos per comoving Mpc^3, and thus an expected number of about 1E-9 halos of this mass in the survey volume. My interpretation of this number would be that finding the halo would essentially be impossible in LCDM. This is in contradiction to the conclusion that "structure may have formed more rapidly than predicted in some regions." Such variations are taken into account in the estimate. It is entirely possible that I made a mistake in my calculation, but it highlights the need for the paper to be clearer on how the statistical disagreement with LCDM was computed.

We appreciate the referee's cross-check of these calculations. Here we summarize the main steps, to show how the presented likelihood is obtained, and explain why it differs from the 1e-9 value derived by the referee.

To compute the footprint, we used the Chandra + JWST overlap of 16 arcmin × 16 arcmin. It appears to us that the referee may have used 16 arcsec × 16 arcsec for the footprint, in which case the volume becomes a factor of 60*60=3600 times smaller. With the correct 16 arcmin x 16 arcmin field and a redshift slice from z=5 to z=7 the comoving volume is V=1.32e6 Mpc^3. As opposed to this, the 16 arcsec × 16 arcsec footprint yields 345 Mpc^3, as the referee found.

Next, we integrated the Tinker et al. (2008) halo mass function in a Planck 2018 cosmology and obtained a cumulative number density of 3e-11 Mpc^-3 for halos with >1e13 Msun. We have verified this result in three ways: (1) using the Python hmf package (2) using the Python colossus package, and (3) manually implementing the Tinker fit. All of these approaches gave the same results. Finally, we  multiplied the survey volume with this cumulative number density, which yields a likelihood on the order of ~1e-4. As a caveat, we note that at this redshift and mass regime halos reside on the exponential cutoff of the mass function, so even small changes in $M_{500}$ can lead to large differences in the predicted halo abundance. We extended the discussion about the details of these calculations.

Another way to see how impossible this halo would be in LCDM is to compute its statistical significance or peak height, which is about 6 for the given mass and redshift (in a Planck cosmology). There are no such halos for all intents and purposes. This would mean that there are two interpretations for a detection, namely that either LCDM is totally wrong or the cluster mass is overestimated. The latter would not be surprising given that we do not know scaling relations at high redshift. I think it would be more interesting to ask what the most massive halo mass is that one could reasonably expect to find in the survey, and how far off known X-ray scaling relations this mass is.

We think that this is an interesting suggestion. Using the referee's idea, we can calculate the mass of the most massive halo that we expect in our survey volume. Using the Tinker halo mass function as above and our survey volume of 1.3e6 Mpc^3, we find that the most massive halo in the survey volume would have a halo mass of ~1e12 Msun. This halo mass is about an order of magnitude lower than that obtained for JADES-ID1. We added this point to the main body of the paper

1b) The authors produce an overdensity map to probe the statistical significance of their detection, namely via converting the galaxy overdensity into a significance. To do so, they "assume 30% cosmic variance," but this number is not justified. Cosmic variance is a sensible quantity only within a certain volume, and no such volume is given. If I understand the calculation correctly, the volume in question should be that of the overdensity, i.e. 250 physical kpc (using the diameter rather than the radius). This would be equivalent to 1-2 comoving Mpc, a scale on which the cosmic variance is a factor of a few rather than 30%. Again, it is absolutely possible that I missed something in this calculation.

To compute the 30% cosmic variance, we did not compute it over the 250 kpc sphere, but over the relevant JWST overdensity footprint and redshift range. In practice, we utilized the method outlined in Trapp & Furlanetto (2020, MNRAS, 499, 2401) as well as their publicly available galcv code to compute the cosmic variance. The input parameters were the area of 3.92 $\mathrm{arcmin}^2$ ($R_{200}$ = 410 kpc of JADES-ID1 at at z=5.7) and the redshift range of 5.44 < z < 6.08. Hence, the survey volume corresponds to a comoving volume of ~7210 $\mathrm{Mpc}^3$. Using the typical brightness of galaxies within the proto-cluster, we obtained cosmic variance of ~35% for $M_{UV}$ = -20 mag and ~21% at $M_{UV}$ = -17mag. In our manuscript, we adopted a 30% uncertainty, which well represents these ranges. In the revised manuscript we provide a more detailed description of this calculation.

1c) A halo mass of 1.3E13 at the given redshift implies a halo radius of R500c ~ 80 kpc. This would place the galaxies highlighted in Fig. 1 within 1-2 R500c, which makes sense (but it would be helpful to plot the estimated halo radius). The galaxy overdensity is measured within 125 kpc and 250 kpc, meaning about 3 R500c. This is fine, but should be stated for clarity.

We updated the manuscript to note that the mass of the proto-cluster corresponds to an $R_{500}$ radius of 80 kpc.

2) A second difficulty I had while reading the paper were the repeated references to "virialization." The authors claim that an X-ray detection is a sign of virialization, but this seems far from obvious to me. Gas is typically heated to the "virial" temperature of halos in accretion shocks. Moreover, the distribution of galaxies shown in Fig. 1 looks fairly irregular. In summary, I am not sure what is meant by "virialization" and what exactly indicates that the cluster (assuming it is one) is virialized. Such a conclusion could be based on a smooth X-ray map, but that does not seem to be the argument the authors are making.

We thank the referee for pointing out our imprecise use of the term “virialization” in the original version. In the manuscript, we intended to convey that the detection of a hot ICM implies that gravitational collapse and shock heating of infalling baryons have begun, but not that it has reached full hydrostatic equilibrium. To reflect this point, we revised the manuscript and discuss the “onset of virial heating” and clarified that this process refers to gas being shock-heated to the virial temperature of the halo. Specifically, we do not claim that the JADES-ID1 proto-cluster “is virialized”, but state that the presence of hot gas indicates that gravitational collapse has begun and that the protocluster is beginning to thermalize its baryons, bringing them up to X-ray temperatures.

3) In Fig. 1, the translation between panels c and d is confusing because a lot of processing happens before the smoothing step. It would be good to show the processed but unsmoothed Chandra map as well. I was also a little confused about the white circles, since the text mentions 66 cluster candidates. The caption only mentions that they mark "likely" cluster members.

We have added a new figure to Appendix A showing the processed but unsmoothed Chandra images before and after excluding X-ray point sources. Here, we also show the locations of the candidate cluster member galaxies within this field.

Referee #3 (Remarks to the Author):

The main result of the paper is the claimed detection of diffuse X-ray emission spatially associated with a high-redshift galaxy overdensity recently discovered with JWST. The authors interpret the diffuse X-ray signal as thermal emission from hot gas, and identify it as the forming ICM of the galaxy protocluster. This result, in principle, would be worth publication on Nature, as it would constitute the first detection of ongoing virialization of a massive halo at z=5.7, i.e. billions of years before the epoch at which most of the galaxy structures are expected to virialize, with a highly surprising and unexpected jump in redshift with respect to the most distant detection of ICM in the X-ray band to date (z=2.15).

However, major technical issues affect the analysis and results, as discussed below, and prevent us from recommending this paper for publication. The main point is that the detection significance of the X-ray diffuse emission and its eventual association with the high-redshift protoclusters are both highly questionable.

Diffuse X-ray emission detection.
This result is based on a relatively straighforward aperture photometry analysis. However, several steps of the analysis carry significant sources of uncertainty, especially in the background level estimate and removal.

- the background level is estimated in a region much larger than, and far away from, the region showing the potential diffuse emission. The Chandra background is low compared to other X-ray facilities, but it is still complex, especially in very deep fields as the CDF-S. Multiple components, both vignetted and unvignetted, concur in producing the total background, and some of them present spatial variations on scales of tens/hundreds of arcsecs. Assuming the average background computed on a large area, which is also spatially distant from the source region, and applying a simple correction based on the average exposure time in such an area does not account for the background complexity. Also, correcting by the exposure map may introduce significant biases, since the instrumental background is not vignetted and should not be corrected according to the exposure map. This issue is especially important for the detection of extended emission with low surface brightness, as that claimed in this paper, for which the background level is more than one order of magnitude larger than the excess signal possibly associated to the diffuse emission.

The reviewer is correct that the Chandra background is made up of multiple components, some of which are vignetted (the unresolved cosmic X-ray background and the Milky Way foreground) and some of which are not (the particle background generated in the detector). While it might seem that one should not apply any exposure map correction to the particle background, in reality the Chandra exposure maps do much more than simply correcting for the effects of vignetting. They also correct for all the detector-specific effects, such as the variations in quantum efficiency across each ACIS-I chip, the bad pixels, and the pattern of chip gaps that inevitably appears when we merge individual observations taken at different roll angles.

Nevertheless, we agree with the reviewer that a more sophisticated background subtraction procedure can strengthen the robustness of our detection and the obtained results.

Following the referee's advice, we updated our procedure to account for the vignetted and non-vignetted background components separately and we also implemented a smaller background annulus. In practice, we produced two sets of exposure maps: one "full" map that includes mirror vignetting and all ACIS-I detector effects, and one "detector-only" map that applies the same detector corrections (chip gaps, quantum-efficiency variations, bad pixels) but does not apply the mirror's vignetting correction. These exposure maps can be generated with the CIAO merge_obs command using the settings units=default (vignetting correction applied) and units=time (vignetting correction not applied). We then split the total background into its physical parts: for the sky background component (unresolved CXB + Milky Way foreground), which is vignetted, we applied the full exposure map, while for the particle background, which is not vignetted, we applied the detector-only map. We find that ~77% of the 0.3-2 keV band background and ~95% of the 3-7 keV band background is particle dominated given that 90% of the CXB is resolved. After subtracting this two component background from the merged Chandra image, the overall results remain effectively unchanged. The extended emission is still detected in the 0.3-2 keV energy range, and no statistically significant signal is present in the 3-7 keV band. In other words, the X-ray signature is still consistent with the presence of hot ICM associated with JADES-ID1.

The revised manuscript describes this background subtraction approach briefly in the main body of the paper and in details in Appendix A. We also revised the inferred values, which, however, did not change significantly.

- As a consequence, the background level estimated from a large region may be affected by unnoticed bias. In addition, the uncertainty on the estimated background in the source region is underestimated, as it does not take into account that the smaller region is affected by stronger background fluctuations.

We agree with the reviewer that a single, large background region could, in principle, miss small-scale variations. To ensure that our detection is not driven by the particular background choice, we repeated our surface brightness extraction using alternative background estimates. Specifically, we varied the inner and outer radii of the background annulus and also subdivided each annulus into 4-8 wedge shaped sectors. In each case, the 0.3-2 keV excess remains detected and no statistically significant emission is detected in the 3-7 keV band. We also note that the surface brightness profiles remain well within our originally quoted uncertainties independently of the applied background. This demonstrates that our results are robust to changes in the background regions and its small scale fluctuations. We updated the paper with this information.

- The assessment of the detection significance should have taken into account that diffuse emission has been searched at multiple positions, corresponding to the (at least) six protoclusters presented in [8]. These counts as six independent experiments, increasing proportially the chance of detecting a positive fluctuation in at least one of them (see, e.g., the Bonferroni Correction method).

Although Li et al. (2024) identified six proto-cluster candidates in the JADES field, only one of them, JADES-ID1, was a realistic target for a Chandra detection of diffuse intracluster emission for the following reasons. First, JADES-ID1 has an estimated halo mass of ~1.3e13 Msun, while each of the other five candidates have halo masses of a few times 1e11 Msun, which is about two orders of magnitude lower. Second, JADES-ID1 hosts 66 potential members, while the other proto-cluster candidates have only 4-19 potential members, which is 3.5-16.5 fewer. At these high redshifts, Chandra with the extremely deep CDFS exposure is only sensitive to the most massive proto-clusters with LX>1e43 erg/s, and none of the other five candidates would exceed that threshold, so a priori we would never have expected diffuse emission from them.

Because only JADES-ID1 met the criteria for a plausible X-ray detection, we did not perform a blind scan of six equally plausible positions. Instead, we pre-selected JADES-ID1 as the only target on physical grounds (its exceptional mass and richness). This was expressed in the original paper:

"*Of these proto-clusters, JADES-ID1 is the most compelling candidate, exhibiting high galaxy overdensity and cluster membership likelihood.*"

In the revised manuscript we further elaborate on this point and the full text now reads:

*"Of these proto-clusters, JADES-ID1 is the most compelling candidate, exhibiting high galaxy overdensity and cluster membership likelihood. A total of 66 galaxies, 14 of which are spectroscopically confirmed, are identified as potential members, with an inferred halo mass of log(Mh/M☉) = 13.28+0.37−0.34, the highest among the JADES fields. Because only JADES-ID1 has the richness and halo mass to produce a detectable ICM signal in the JADES field, our X-ray analysis targets this proto-cluster candidate."*

In statistical terms, we fixed the location of our search a priori based on the independent JWST evidence. When the search location is fixed in advance, the local p-value already correctly reflects the significance and a look-elsewhere correction is not needed (Gross & Vitells 2010; Protassov et al. 2002). Moreover, since the trial factor is N=1 (as only one hypothesis was conducted), the Bonferroni correction is not applicable. We revised the manuscript to make the above points clear.

- The authors have checked for extended emission in the 3-7 keV, in addition to the 0.3-2 keV band. It remains unexplained why the 2-3 keV has been discarded. One explanation might be that the Chandra effective area varies strongly in that band, but the same is true for the 0.3-2 keV energy range.

We do not include the 2-3 keV energy range in our analysis because this band is dominated by instrumental fluorescent lines. Specifically, the Au L complex plays a major role in this band, with line energies between 2.1 keV and 2.4 keV, for details, we refer to Suzuki et al. (2021, A&A, 655, A116). These gold lines arise from fluorescence in the ACIS detector coating and have spatial patterns that differ from the rest of the particle background. We also refer to Bartalucci et al. (2014, A&A, 566, A25), whose work presents an analytical model of the ACIS-I “stowed” background and explicitly identifies the Au line complex at ~2.1-2.4 keV. We note that this line complex is by far the strongest feature at energies <7 keV and exhibits its own spatial and temporal variability. For these reasons, and in agreement with standard best practice when studying faint emission (see e.g. Bartalucci et al. or Hickox & Markevitch 2016, ApJ, 645, 95), we omit the 2-3 keV band from our analysis to avoid contamination by these instrumental lines. We have updated Appendix A of the manuscript to explicitly note this point.

- As a check, we performed an aperture photometry analysis similar to the one done in this manuscript, but in the 2-7 keV band. Assuming a more local background than that used in the draft, we found a positive fluctuation of about 400+-100 net counts. If confirmed, this would suggest a positive fluctuation in the instrumental background in that area (affecting both bands) rather than a signal from ICM emission. In fact, a hard detection at z~6 would be extremely hard to reconcile with thermal emission.

To further check for any hard band emission associated with JADES-ID1, we repeated our aperture photometry analysis in the 3-7 keV band, which avoids contamination from the strong Au L complex, which has spatial patterns that differ from the rest of the particle background (see our response above). To this end, we used the same 21 arcsec radius source region but estimated the net counts using various local background annuli, including annuli with different radii and wedge-shaped sectors, to capture spatial variations in both particle and sky background components. When accounting for the background components, we applied our method involving two exposure maps to separate vignetted (sky) and non-vignetted (instrumental) contributions. As we explained above, the full vignetted exposure map is used for the sky background components while the detector-only map (no vignetting correction) is used for the particle background. With every choice of background region and map combination, we do not detect a statistically significant signal in the 3-7 keV band. This robust non-detection in the hard band excess is exactly what one expects for a few-keV thermal plasma at z = 5.7.

- The authors have not searched for emission at a specific position, but rather on an area "close enough" to the claimed protocluster center. Not many details are given on this point - e.g., what is the maximum distance between the diffuse X-ray emission and the protocluster center that the authors would accept to claim a physical association? The detection significance should take into account that positive fluctuations could have occurred over such an extended area. Instead, it is implicitly assumed that the authors looked at only one specific position and found the diffuse emission.

We did not perform any type of sliding-box search to identify the brightest X-ray patch near the JWST identified coordinates. Instead, we fixed our aperture at the single sky position defined a priori by the highest-likelihood JWST overdensity peak defined by Li et al. 2024. At that coordinate, we carried out straightforward aperture photometry. As for the physical origin of the potential offset between the JWST and the X-ray peak, we provide a detailed explanation below to address another question of the referee.

The quoted 5 sigma detection significance (combining the 0.3-2 keV excess, the 3-7 keV non-detection, and the radial profile) corresponds to the probability that a single background fluctuation within that fixed 21 arcsec aperture would mimic our observed signature. We emphasize that not only our search location but also our aperture was physically motivated, namely to match the expected virial radius of a (1-2)e13 Msun proto-cluster at z=5.7. Therefore, we believe that no additional correction is required for a look-elsewhere effect as we also explain above.

- Individual point sources are detected with the wavedetect tool and masked. The text report the detection of only one point source in the region of the diffuse emission, while dedicated source detection procedure (e.g., Luo+17) report the detection of at least two point sources at a few arcsec separation. While these are relatively faint sources, they can still contribute to the very faint diffuse emission. The paper does not report explicitly the position of the detected point source and the radius used to mask it, so further checks are not possible. Fig. 1 should have showed the X-ray image before and after the point source masking, and the regions used for masking, for clarity. Moreover, more details on the point-source masking/refill procedure whould be given. E.g., what statistics has been used during the masked region filling?

Following the referee's suggestion, we have revised our point source masking analysis to also include the fainter and lower significance sources reported by Luo et al. (2017). We note that these additional sources are indeed faint and not particularly soft, so they do not affect the soft, extended emission associated with JADES-ID1. We have extended Appendix A with our revised point source detection approach, which included all faint sources from the Luo et al. (2017) catalog. We note that the Luo et al. (2017) catalog also contains faint point sources within our background region that were not masked in the original analysis. In the revised manuscript, we have excluded these sources as well. Because these fainter sources are distributed roughly uniformly across the CDFS field, they affect the source and background surface brightness in a similar manner. Consequently, even after excluding these faint sources, the overall detection significance of the JADES-ID1 proto-cluster and our main conclusions remain unchanged.

In the updated manuscript, we include a new figure (Fig. A1) in Appendix A that shows the merged, unsmoothed 0.3-2 keV band Chandra image before and after masking the point source. In this figure, we also overlay the exclusion region of all point source regions, along with the locations of the z ~ 5.7 candidate cluster member galaxies. As we noted in our response to Referee #1, the smoothed image is used only for visualization purposes and all quantitative analyses rely on the unsmoothed and point source masked images. For clarity, we also note in the revised manuscript that when filling in the masked regions, we used the CIAO dmfilth tool in "global" mode, which replaces masked pixels by drawing Poisson random values whose mean equals that of the background pixels.

- The statistical error on the 144 net counts should be reported, as it already gives a sense of the detection significance. But see our comment on the severe underestimate of the uncertainties associated to the background subtraction.

We added the statistical uncertainty on the observed net counts.

- The position of the peak of the diffuse X-ray emission should be reported explicitly, to allow reproducibility.

We have added the coordinates of the diffuse X-ray emission peak (RA=3:32:31.75, Dec=-27:46:51.5) in the main body of the revised paper.

- Fig. 2 presents the radial fit with a beta model of the claimed diffuse X-ray emission. More details should be given here to clarify the followed procedure, including the position of the radial center. In particular, it is unclear whether the smoothed or unsmoothed radial profile has been fitted. In the former case, the signal is artifically spread over nearby pixels, introducing a spatial correlation which is not present intrinsically. Any background fluctuation would have a similar radial profile. However, the most important issue here is that the authors fitted the background-subtracted signal (whether smoothed or not), and thus do not account for the background uncertainty. This is worrying, as the background counts are expected to be more than ten times higher than the signal counts.

The smoothed X-ray image of the diffuse emission, presented in Fig. 1, is only used for illustration purposes and it is not used for any quantitative analysis. We revised the text to clarify this important point. All quantitative analysis, including the surface-brightness profile shown in Fig. 2, was performed on the unsmoothed 0.3-2 keV band image where the point sources were masked and their location was not filled. In the revised manuscript, we also specify the center used to extract the X-ray surface brightness profile.

For each annulus, we subtract the background level measured in the 40"-110" annulus, and the uncertainty on the background-subtracted counts is computed from the Poisson error on the total counts in that bin, which at sufficiently high counts numbers like in our case can be approximated as $\sigma \approx (N\_total)^{0.5}$ (Gehrels 1986). This procedure fully propagates the dominant background uncertainty and produces the increasing error bars at larger radii as the signal-to-noise declines. As discussed above, our results are independent of the specific background region used. Indeed, we tested different annuli as well as wedge-shaped sectors as background regions, and our main conclusions (detection of soft band emission, non-detection of hard band emission, declining surface brightness profile) remained unchanged.

- The method used to create Fig.3 and presented in Appendix C should be described in detail and more explicitly. In any case, Fig. 3 is used to support the detection of the diffuse emission close to JADES-01. The authors state that other positive peaks are visible in the figure, but at lower significance levels. For example, the signal at RA=3h32m35s and DEC=-27d46m30s appears to be comparable to, if not stronger than, the signal reported for JADES-01 when integrated over a suitably large region. Finally, following our previous note that any background fluctuation would have a similar radial profile when smoothed, we argue that combining the 0.3-2 keV excess with 3-7 keV non detection (but see our note that 2-7 keV images does have an even larger excess) and with the radially declining profile, would artificially enhance the significance of the detection. This cast doubts on the use of this map to visually represent the detection significance of the extended emission.

Each pixel location in the likelihood map is the centroid for the circles/annuli used extraction region to calculate the combined probability described in Appendix D (Appendix C in the previous draft). Since the pixel resolution of the likelihood map is ~2.5” (5 Chandra pixels) and the smallest extraction region is 6”, the pixels are not statistically independent due to the oversampling. However, each pixel value reflects the probability of (1) significant counts above background within 6”, (2) a rising profile from 6-10” annulus and 0-6” annulus, and (3) a non-detection in the hard band within 10”. Integrating (summing) over multiple pixels will not get a meaningful value, as in this case one would add regions that are not statistically independent.

Association of the diffuse X-ray emission with the JADES-01 galaxy structure.
Under the assumption that the diffuse X-ray emission is real, its association with the actual galaxy structure at z=5.7 remains poorly justified.

- The method used to estimate the center of the galaxy structure is not described in this paper. The reference paper ([8]) is accepted but still unpublished, and the list of galaxies considered members of the structure is not reported in the online version. An unknown number of the galaxies are not confirmed spectroscopically. We assume that the structure center is simply the average position of its members. This estimate is clearly quite rough. Moreover, it does not account for the possible member galaxies that are located outside the JADES field of view, as similar structures at high redshift can extend to several arcmins (as also shown in Fig. 13 of [8]). Therefore, the position of the "center" of the galaxy structure is highly uncertain.

We thank the referee for raising the question of how the proto-cluster center is defined. We note that our reference paper (Li et al. 2025, MNRAS, 539, 1796) is now published and we have updated the citation accordingly in the revised manuscript. Moreover, we mention in the revised manuscript that 14 of the 66 candidate member galaxies are spectroscopically confirmed, while the remaining galaxies satisfy our photometric-redshift and SED-quality criteria.

To derive the proto-cluster center, we did not use the geometric mean of the member positions. Instead, we construct a two-dimensional galaxy-overdensity map in narrow redshift slices ($\Delta z$=0.1), using the DETECTIFz algorithm (Sarron & Conselice 2021, MNRAS, 506 2136), and used Monte Carlo realizations of each galaxy’s redshift probability distribution function. The adopted centroid for JADES-ID1 is the pixel value and the corresponding RA and Dec with the highest local overdensity in the $z \approx 5.68$ slice. We have updated the manuscript to clarify this. We have added a brief description of the method to determine the centroid to the main body of the paper. For the full details, we refer to Li et al. 2025 and Sarron & Conselice 2021.

While we acknowledge that this approach does not account for potential members outside the field of view as high-redshift structures can indeed extend over several arcminutes, the available JWST images focus on the well-sampled core of the system.

- The galaxy overdensity levels computed on different scales do not exceed 4sigma, which is a low value for a core of a massive galaxy structure (although it is a strong overdensity for being at z=5.7). The fact that the overdensity level does not increase significantly when computed on smaller scales again suggests that there is not a clear core position.

At z>5 the observed significance of galaxy overdensities is naturally expected to be lower than at lower redshifts, due to the lower galaxy number densities, rising observational incompleteness, and the ongoing formation of cosmic structures. Cosmological simulations (e.g. Chiang et al. 2013, ApJ, 779, 127; Overzier et al. 2016, A&ARv, 24, 14) show that high-redshift proto-clusters often lack a well-defined density peak or core, and member galaxies can be dispersed across Mpc scales. Therefore, at z ~ 5.7 even a ~4 sigma overdensity over a few arcmin$^2$ is both rare and significant, and the absence of a more pronounced central peak is not concerning. This conclusion is further supported by the fact that the inferred halo mass exceeds theoretical expectations for this redshift by an order of magnitude (see our response to Referee #2). In the revised manuscript, we discuss that the relatively flat overdensity profile and absence of a sharply peaked core are in fact fully consistent with a dynamically young, still-collapsing proto-cluster.

- Even assuming that the protocluster center used in this paper is the actual center, the spatial shift with the diffuse X-ray emission likely indicate a chance association, and thus a likely spurious (or foreground) nature of the X-ray emission. The authors address this point in the paper by refering to the young age and on-going virialization of the protocluster, but such an explanation is qualitative and speculative. We also stress again that the combined likelihood of the galaxy structure + diffuse X-ray emission has been computed assuming they are spatially coincident, while they are not.

We disagree with the referee that the ~8” offset implies a spurious or chance association between the X-ray emission and the JWST-detected galaxy overdensity. In fact, as we argue in the paper, there are multiple lines of evidence suggesting that the X-ray detection is physically connected to the proto-cluster ICM.

The diffuse X-ray emission is spatially coincident with the highest galaxy overdensity and the most likely proto-cluster centroid identified by JWST. Although the X-ray peak and the galaxy overdensity peak is slightly offset, the bulk of the X-ray emission is co-spatial with the region with the highest cluster membership. As we discuss below, the offset of ~47 kpc does not exclude that the galaxy overdensity and the diffuse X-ray emission have a physical association. Indeed, merging and dynamically young galaxy clusters often have similar or even larger offsets.  For example, the ICM associated with the Spiderweb protocluster exhibits an offset of ~50 kpc from the central radio galaxy, yet is clearly the same structure (Di Mascolo et al. 2023, Nature, 615, 809). Similar offsets between the optical and X-ray center are routinely observed in low- and high-redshift clusters (see our response to the next question).

We also investigated the X-ray data and probed if it contains any discrete source that could explain the origin of the X-ray emission. As we discuss in the new Appendix C and in our response above (see also our response to Referee #1), the contribution from residual emission from point sources cannot explain this emission. Moreover, the X-ray emission appears soft and diffuse. If background AGN were responsible for the observed emission, we would expect a multitude of bright point sources and we would expect a harder X-ray spectrum (i.e. detection in the 3-7 keV band), which is not observed for JADES-ID1.

A chance superposition of a foreground group is also unlikely because JWST did not identify a concentration of low-redshift galaxies at the location of JADES-ID1. If a low-redshift galaxy group was present, the deep and sensitive JWST observations could have easily picked up these galaxies in the JADES field.

We also note that the observed physical properties, namely its luminosity and inferred temperature, of the JADES-ID1 proto-cluster are also consistent with those expected from its ICM at $z \approx 5.7$. Indeed, the best-fit thermal plasma model points to $kT{\sim}2$ keV, which is expected for an assembling proto-cluster at this epoch.

Taken together, these arguments make a compelling case that the X-ray detection is genuinely the nascent ICM of JADES-ID1, rather than a spurious or unrelated feature.

- The fact that the center of the extended emission is not associated with a high density peak in the density distribution, is in strong disagreement with any reasonable expectation for proto-ICM at high redshift. Indeed, at much lower redshift ($z{\sim}2$) ICM emission is always associated with a beacon galaxy or a clear galaxy overdensity on small scale, in line with the expectation that the diffuse baryons are heated to >0.5 keV level in collapsing massive dark matter halos already associated to a significant stellar mass. Also, dynamical decoupling of diffuse baryons and galaxies is not expected at low overdensities (delta~4 for JADES-ID1) where the peculiar velocities are low.

For fully virialized relaxed clusters it is indeed expected that the peak of the ICM distribution coincides with the center of the galaxy distribution, which is usually marked by the Brightest Cluster Galaxy (BCG). However, in a young protocluster at $z \approx 5.7$ with a relatively low overdensity, an exact spatial coincidence is not guaranteed and not expected. Simulations, such as TNG or Magneticum, predict that during early stages of cluster assembly, the gas may still be infalling along filaments or sloshing within a dynamically young potential, which, in turn, could lead to substantial offsets between the gas density peak and the overall mass/galaxy centroid (Seppi et al. 2023, A&A, 671, 57). In other words, the ICM is not fully virialized to the dark matter and galaxy center at such high redshift. This theoretical expectation is supported by a range of observational studies at both low and high redshifts.

Nearby merging clusters are perhaps the best illustration that the ICM emission can be significantly displaced relative to the galaxy overdensities. Perhaps the most famous example is the Bullet cluster (1E 0657-56), in which the hot gas was ram-pressure stripped during a high-speed collision and lags behind the collisionless galaxies and dark matter. The X-ray centroid in the Bullet cluster is offset by ~100 kpc from the galaxy mass peak (Markevitch et al. 2004, ApJ, 606, 819). In more general terms, surveys of local galaxy clusters find that disturbed, merging systems tend to show tens to hundreds of kpc separation between the X-ray center and the optical center, while relaxed clusters show good agreement between the X-ray center and the optical centers. In fact, these offsets can even be used as an indicator of dynamical young systems or ongoing mergers. The observed ~47 kpc separation for JADES-ID1 is well within this range observed in the population of unrelaxed clusters.

To illustrate the above point with larger cluster samples, we list a few studies investigating the offset between the X-ray centroid of the ICM and the offset of the BCG. First, we highlight the recent eROSITA study of massive clusters that found an average X-ray/optical offset of 76 kpc, with larger typical offsets in disturbed systems (Seppi et al. 2023, A&A, 671, 57). Similarly, a Chandra study of 108 massive galaxy clusters in the redshift range of $0.15 < z < 0.7$ compared X-ray peaks and the position of BCGs and found that 25% of merging clusters had offsets of >50 kpc (Mann & Ebeling 2012, MNRAS, 420, 2120). Additionally, a Chandra study of a large Planck-selected cluster sample revealed that mergers can produce X-ray/optical offsets of several hundred kpc (Rossetti et al. 2016, MNRAS, 457, 4515). Thus, the observed 47 kpc offset in the JADES-ID1 proto-cluster is entirely plausible if the protocluster is undergoing a merger or has an unrelaxed core, as expected from a forming proto-cluster at high redshift.

At higher redshifts, proto-clusters are in earlier phases of their assembly. For these proto-clusters similar centroid offsets have been observed. One striking example is the Spiderweb proto-cluster at z=2.16. Deep X-ray and Sunyaev-Zel'dovich (SZ) observations revealed X-ray-emitting ICM whose SZ centroid is offset by about 6" (~50 kpc) from the location of the Spiderweb Galaxy, which is the central radio galaxy (Di Mascolo et al. 2023, Nature, 615, 809). This offset provides evidence for an inhomogeneous, merging ICM distribution rather than a fully settled cool core. In fact, the multiwavelength analysis of the Spiderweb system suggests that this is an actively collapsing cluster, where the offset between the SZ signal and the galaxy density peak indicates a dynamically young structure. The situation in our protocluster in the JADES-ID1 proto-cluster at $z \approx 5.7$ is likely similar to that in the Spiderweb proto-cluster, but observed at an even earlier epoch of cluster formation. Since this system is observed merely ~1 billion years after the Big Bang, JADES-ID1 is likely in the phase of rapid assembly, implying that ongoing mergers between individual sub-clumps can displace the hot gas from galaxy distribution.

In summary, both simulations and observations demonstrate that the peak of the ICM emission does not need to be centered on the galaxy overdensity in a dynamically young or merging cluster. The observed offset of ~8" (~47 kpc) is physically reasonable for a protocluster that is in the formation stage and is undergoing collapse and mergers. We revised the text to reflect these points.

Referee #4 (Remarks to the Author):

I co-reviewed this manuscript with one of the reviewers who provided the listed reports.

The authors thank the reviewers for their continued engagement and very helpful suggestions which have greatly strengthened and improved the paper. We believe that the additional analyses and statistical tests suggested by the referee have made our detection of the z≈5.7 proto-cluster even more robust than our already careful analysis. Below, we address each comment made by the reviewers and indicate the corresponding changes in the manuscript in blue font. In addition to these changes, we have also moved certain technical details from the main text into the Methods section to follow Nature's formatting guidelines.

Sincerely,
Akos Bogdan
Gerrit Schellenberger
Qiong Li
Christopher Conselice

Referee #1 (Remarks to the Author):

I would like to thank the authors for the detailed responses and for taking my comments into consideration. I think the revisions have significantly improved the manuscript, and have addressed the concerns I raised in my previous review.
I particularly appreciate the inclusion of the novel appendix that explores potential alternative origins of the observed X-ray emission.

Despite this, after reading the comments by Referee #3 and the respective responses by the authors, I would like to express a moderate concern about the interpretation of the likelihood map and any potential biases due to foreground structures.
The authors state that only the signal in the direction of JADES-ID1 "satisfies all of our X-ray criteria ... and is co-spatial with the independently identified JWST galaxy overdensity". Although this is clearly and effectively testing whether the signal could be statistically consistent with being associated with the z=5.7 complex, it is not testing against the presence of a foreground structure. In the rebuttal, the authors state that no low-redshift groups were identified in the JWST data, but how this was determined nor from where such an information was retrieved. For instance, the reference paper [8] does not provide a detail discussion on the level of contamination for each considered system.
Addressing such a point with a direct and quantitative test would be beneficial to the manuscript, and would significantly strengthen the reported finding.

We thank the referee for raising this important point about potential foreground contamination and our interpretation of the X-ray likelihood map. Addressing this point is critical for our analysis so we have now looked into these points in some detail. To address it quantitatively, we have now performed an independent test using the JWST + HST photometric catalog of the JADES field.

We mapped the redshift-dependent galaxy surface density within a 40" x 40" box (within which region we detect the X-ray emission associated with JADES-ID1) centered on the X-ray

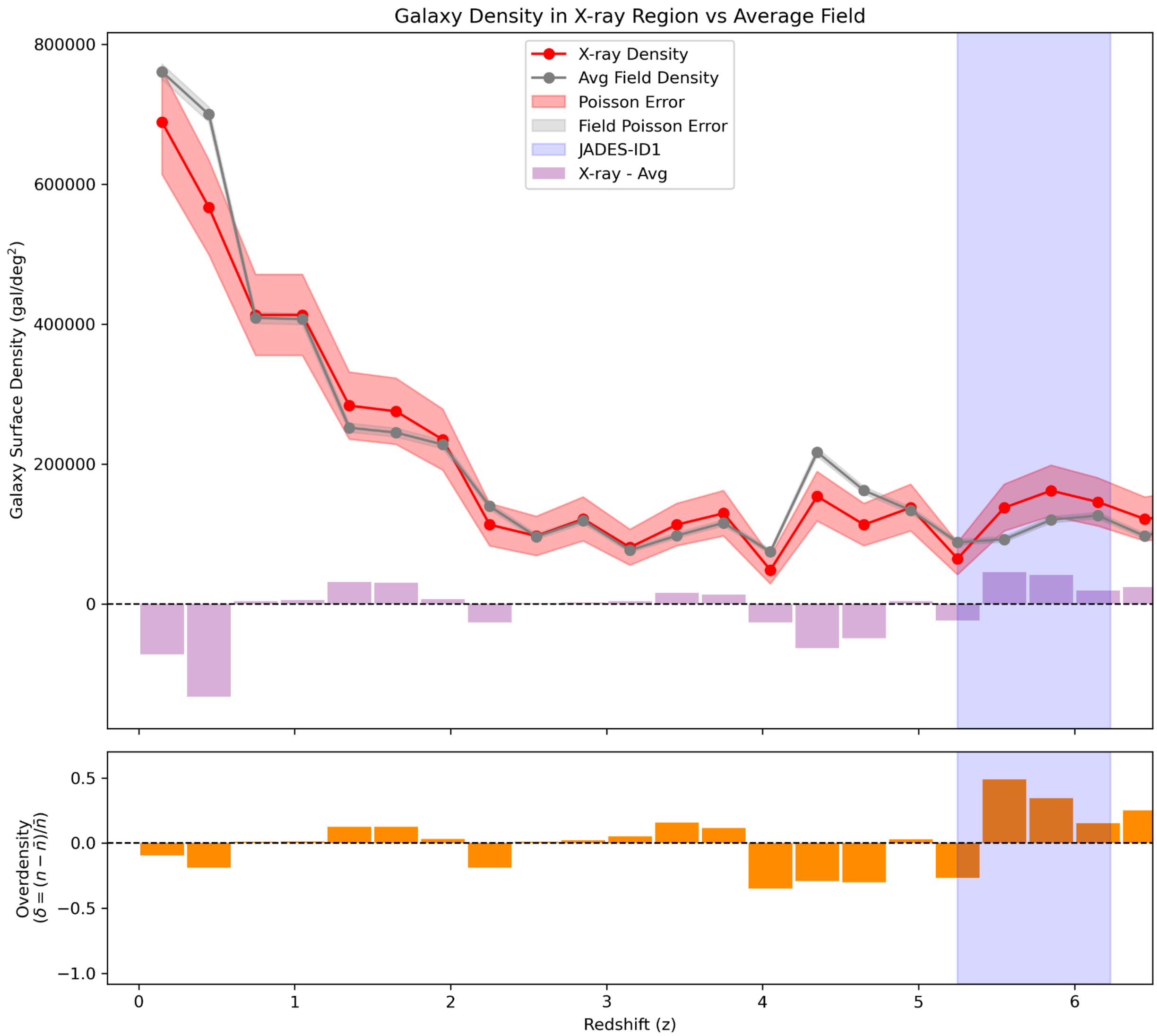


Figure 1: *Galaxy surface density as a function of redshift for the JADES-ID1 region region (red data points) and the average field (gray), based on the JWST + HST photometric catalog of the JADES field. The purple bars show the difference in number density between the two. Poisson errors are shown as shaded bands. The light blue vertical band highlights JADES-ID1 redshift range, where the only positive overdensity appears.*

emission peak. We divided the z=0-6.6 redshift range into redshift bins with width of $\Delta z = 0.3$, counted galaxies in each bin, and normalized by the area. To control for selection effects (e.g. most notably the increased detectability of low-redshift galaxies), we measured the same distribution in a large background region in the JADES footprint, while specifically excluding the 40" x 40" box where we detect the X-ray emission associated with JADES-ID1. Figure 1 shows the resulting density in the X-ray region (red points), the field average (gray points), and their difference (purple bars), with Poisson error ranges shown with shaded regions. The only significant overdensity occurs within the z=5.25-6.23 redshift range (purple shaded band), where JADES-ID1 cluster members are detected, and there is no comparable excess at any lower redshift bin. This analysis confirms that the X-ray excess cannot be attributed to any low-redshift group or cluster. The photometric redshift distribution of the JADES-ID1 region and its direct comparison to the JADES field points to the z≈5.7 overdensity, supporting our conclusion that the X-ray emission is physically associated with the JADES-ID1 proto-cluster.

We have added a description of this analysis to the new Section 1 of Methods, where we also moved the description of the more technical procedure that was used to derive the JADES-ID1 centroid. This analysis further makes it very likely that the X-ray structure we see is real and associated with the high redshift proto-cluster.

And a minor comment: there might a typo in Eq. D1 (5" in the third term instead of 6")

We thank the referee for catching this typo. The third term in Equation D1 should indeed be 6" (not 5"). We have corrected the equation accordingly.

Referee #2 (Remarks to the Author):

I appreciate the care taken by the authors to address my comments. I would be happy to recommend the paper for publication if a few smaller points below would be addressed. This is not meant as a statement about the more observational issues raised by the other referees.

1) Regarding the calculations:

1a) My apologies for the silly mistake, I did indeed use 16'' instead of 16'. With that fixed, my calculation of the likelihood of finding a halo of the given mass in a the survey volume gives a slightly lower value, namely 3E-5 for a 1E13 solar mass halo and 4E-7 for a 1.8E13 halo (the value quoted in the abstract). The exact values depend on the Hubble constant assumed and such, so the difference is not a big worry.

We thank the referee for recalculating the likelihoods with the correct Chandra field of view. At z = 5.7, the inferred halo mass of JADES-ID1 lies on the exponential tail of the halo mass function. Therefore, even a modest change in $M_{halo}$ can produce a substantial difference in the calculated likelihood. This is well illustrated by the fact that a factor of two change in halo mass (1e13 $M_{sun}$ vs. 2e13 $M_{sun}$) leads to roughly a two order of magnitude difference in the derived likelihood. Moreover, small changes in the assumed cosmology can also change the obtained likelihood values, as pointed out by the reviewer.

In the first revision, we provided an order of magnitude estimate for a $10^{13}$ $M_{sun}$ halo mass given these points. However, to be more precise in the revised manuscript, we specify the cosmology paper we used (Planck Collaboration 2018, 2020, A&A, 641, A6 (Paper VI), Table 2 (TT, TE, EE + lowE + lensing + BAO) and then quote the value of 4 x $10^{-5}$ for the $10^{13}$ $M_{sun}$ halo mass, which is in good agreement with the reviewer's calculation. For the upper bound, we prefer to retain the 2 x $10^{13}$ $M_{sun}$ case as an illustrative upper value, to show how the probability varies given the potential range of halo masses.

1b) I appreciate the explanation of the cosmic variance calculation. It makes sense that the CV in the UV luminosity function with the given magnitude limits is about 30%, but I wonder if this is the most salient question to ask in the present context? Another question is to what extent we should expect JADES to give an unbiased representation of the universe. For example, the likelihood of a 1E13 halo forming will drastically increase in regions with large-scale overdensity. The relevant quantity would be the variation of background density in volumes of JADES' size. This can be computed by integrating the non-dimensional variance based on the linear power spectrum.

We agree with the referee that the probability of forming a $10^{13}$ $M_{sun}$ halo is higher in an overdense region. In addition to our previous discussion on cosmic variance in the UV luminosity function, we followed the referee's suggestion and estimated the variance in the underlying matter density field over our JADES-sized volume. To do so, we computed the r.m.s fluctuation in the matter density field,

$$\sigma^2 = \frac{1}{2\pi^2}\int_0^\infty k^2 P(k)\, |W(kR)|^2\, dk,$$

where P(k) is the linear power spectrum at z=5.7, and W(kR) is the top-hat window function. To this end, we use the JADES footprint and the redshift slice from 5.25 to 6.23, which we convert into a comoving volume of 70,000 $Mpc^3$. We thus obtain the scatter in the matter density field, $\sigma(R) \approx 0.059$, which implies a 6% fluctuation from one JADES-sized field to another. Based on the Tinker (2008) halo mass function, a 6% overdense field implies a boost of the abundance of $10^{13}$ $M_{sun}$ halos by a factor of ~6. However, while the probability of detecting a $10^{13}$ $M_{sun}$ halo in an overdense, JADES-sized field increases from 4 x $10^{-5}$ to 3 x $10^{-4}$, it is still highly unlikely as it would be 3.6σ instead of 4.1σ. We have added a brief summary of this result to the main text, with a more detailed discussion in Section 1 of Methods.

2) Regarding the interpretation of the results:

2a) In line 247, the paper states "At these redshifts, the universe is predicted to host only a few dozen of systems with..." Do you mean the observable universe? The universe might be infinite.

The referee is correct. We have revised this sentence to specify that we are referring to the observable universe.

2b) I also wonder if, in addition to the cosmic variance calculation, one might need to worry about the look-elsewhere effect with survey volumes as small as JADES. After all, the present paper would presumably not have been written had there not been a tentative detection. I realize that this effect is almost impossible to quantify, but it adds another caveat to a rigorous statistical interpretation.

This is an interesting and important almost philosophical point that is important in many different areas of high redshift studies where “unusual” objects are found. While we addressed the “look-elsewhere” effect in our earlier response, noting that JADES contained multiple proto-cluster candidates, considering survey volume is indeed a complementary angle. We agree with the referee that testing many independent volumes or apertures can inflate the chance of a random fluctuation. However, in our case this is not a concern for two main reasons.

First, our X-ray search was a priori hypothesis-driven by the JWST galaxy overdensity at z~5.7. As we explained in our previous response, we did not carry out a blind scan of multiple regions or redshift bins, so there is effectively only one trial, driven by the JWST identification of JADES-ID1. As noted in our previous response to Referee #3, although the JADES field contained several candidate overdensities, all but one (JADES-ID1) have substantially fewer member galaxies and JWST-inferred halo masses at or below the threshold for forming X-ray-emitting halos. Thus, JADES-ID1 was the only realistic proto-cluster candidate in this survey volume.

Second, while JWST has identified proto-cluster candidates in other deep fields (e.g. Li et al. 2025, MNRAS, 539, 1796 found multiple proto-cluster candidates in the CEERS and NEP fields), these fields lack the necessary X-ray depth and the Chandra observations with low molecular contamination on the ACIS optical blocking filters. The CEERS field has only about 10-20 % of the exposure time of CDF-S (depending on the position) and misses the earliest Chandra cycles when molecular contamination was minimal. The NEP field, with its 1.7 Ms (~24 % of CDF-S), consists entirely of data taken late in the Chandra mission (Cycles 19-24) when contamination blocked 95–98 % of photons with <1 keV energies (Plucinsky et al. 2018, SPIE, 10699, 6; Plucinsky et al.  2022, SPIE, 12181, 6). Consequently, neither field can reveal the faint, redshifted ICM emission we target in our study.

In summary, a field suitable for detecting high-z proto-cluster X-ray emission needs to combine extremely deep observations, significant early-mission coverage, and no foreground contaminants such as bright galaxies or clusters that would elevate the overall “background” level. The CDF-S field (i.e. JADES in the JWST nomenclature) is, unfortunately, the only survey volume that meets all these requirements. Therefore, there is no sample of independent, equally viable fields with JWST *and* deep Chandra data that would justify invoking the look-elsewhere correction. For now, the JADES/CDF-S field is uniquely positioned to reveal X-ray emission from a genuine, high-redshift proto-cluster. In fact, this overdensity may be just the tip of the iceberg in terms of the discovery of many similar systems which if indeed the case would certainly alter our entire view of structure formation in the universe.

Given these limitations, we truly look forward to the next generation of X-ray and SZ observatories with their greatly improved sensitivity, that will allow the joint follow-up of the exciting proto-cluster candidates that are being uncovered by JWST.

We have also added a sentence to the second paragraph of the main body to highlight the uniqueness of the JADES/CDF-S field.

2c) In line 258 and following, the authors connect the proto-cluster detection to the detection of "over-massive" galaxies in JWST. I think we should be careful with this analogy: so far, JWST galaxies can be explained by LCDM structure formation and very efficient star formation. While this also "challenges our understanding of early structure formation," it seems like it does not challenge LCDM itself but mostly our understanding of baryonic galaxy formation physics. In contrast, detecting hot gas from a 1E13 halo could presumably NOT be explained by some modification to baryonic physics, given that the physics of shock heating and gravity is relatively simple. I think it would be good to very briefly make this distinction.

We thank the reviewer for highlighting this distinction. We agree that this is an important point, and we have updated the second to last paragraph of the main text accordingly.

Referee #3 (Remarks to the Author):

The authors have not addressed completely the two most relevant comments, namely 1) the significance of the diffuse soft-band X-ray emission and 2) the physical association with JADES-ID1, as reported below.

Detection significance:
According to the new information provided by the authors on the uncertainty of the net counts, the significance of the 0.3-2 keV detection is about 3sigma. This is the core of the results presented by the authors, before other ancillary information on the lack of hard-band emission

and possible spatial distribution are accounted for. A 3-sigma detection can be considered a good indication, but hardly a conclusive result. The derivation of the radial profile of such faint emission and the fit with a beta model are even more delicate issues.

We thank the referee for this additional comment which we now more fully address. Detecting the hot ICM emission from proto-clusters at z > 5 with today's instrumentation pushes and often exceeds their limits. In the case of the JADES-ID1 proto-cluster, however, it is the ensemble of evidence that makes our interpretation compelling. Deep JWST near-infrared imaging identified a statistically significant overdensity of galaxies at z~5.7 providing a clear a priori target for our X-ray study. Our Chandra analysis then revealed an X-ray excess in the 0.3-2 keV band, with no hard band counterpart, which is the expected signature of shock-heated gas with kT~2 keV temperature redshifted to z~5.7. This emission is spatially extended and its extent and morphology is in good agreement with high-resolution simulations of early cluster assembly. Further consistency checks ruling out low-redshift foreground contamination (see our response to Referee #1) and recovering similar signals when splitting the data by epoch or at random (see our answer below), demonstrate that no single line of evidence stands alone. Instead, the photometric, spectral, morphological, and statistical results together paint a unified and self-consistent picture that JADES-ID1 indeed hosts a genuine, shock-heated ICM, opening a new frontier in the study of the earliest stages of cluster formation.

One test that the authors can carry out to reinforce the detection claim is to split the observations into two sets with similar exposure times, and repeat their procedure. If the emission is real, they will detect it at about 2sigma in each set, otherwise a background fluctuation is more probable.

We agree with the referee that this is an excellent test to verify the robustness of our detection and rule out a fluctuation driven by just a handful of observations. As a potential caveat of this approach, we note that the CDF-S Chandra data span a 15 year baseline, during which time the ACIS optical blocking filter accumulated a substantial amount of molecular contaminants. This contaminant preferentially absorbs soft X-rays, so observations taken earlier in the mission (with less contamination) yield higher net counts for the X-ray emission associated with the redshifted, soft-spectrum proto-cluster. To account for the effects of the contamination, we reanalyzed and split the Chandra observations of the CDF-S field in two complementary ways:

(1) Sequential by observation date
We divided the observations chronologically into two groups with approximately equal total exposure times (~3.3 Ms). The first group contains early-epoch (2010 and before) observations with less molecular contamination, while the second group contains observations taken in or after 2010 with higher molecular contamination.As expected, the first group (less affected by contamination) yielded both a higher count rate and greater significance than the second group. Approximately 65 % of the total net counts associated with JADES-ID1 originate from the first half and only 35% of the total net counts originate from the second half. This is consistent with our understanding of the contamination build-up and its effects on the observed soft counts.

Moreover, this result further supports the soft spectral characteristics of JADES-ID1, since the more sensitive early Chandra observations collected the bulk of the X-ray photons.

(2) Randomized by observation ID
As a second approach, we randomly assigned observations into two groups and once again the exposure time of each group was approximately the same. This randomization removes any systematic bias associated with the buildup of the molecular contaminant. Therefore, we expect that the net counts associated with JADES-ID1 will be evenly distributed between the two groups. We repeated this analysis multiple times with different random subsets and consistently found that the net counts split approximately 50:50 between the two subsets.

Overall, this analysis confirms that the X-ray emission associated with the JADES-ID1 proto-cluster is neither dominated by a few epochs with unusually high exposures nor driven by temporal variability. This strongly supports a genuine, extended X-ray source rather than a background fluctuation. We added a brief summary of this analysis to Section 2 of Methods.

Physical association with JADES-ID1:
Even assuming that the diffuse emission is real, its physical association with JADES-ID1 remains highly uncertain. The authors refer to the fact that simulations predict irregular gas distribution. Using the same reasoning, the same simulations do not predict significant heating of such gas during the phases of infall at high redshift.

Cosmological simulations of proto-clusters show both irregular, filamentary gas distributions and strong shock heating of accreting material in the most massive halos at high-redshifts, such as JADES-ID1. For example, Costa et al. (2014, MNRAS, 439, 2146) generated simulated X-ray maps of a z=6.2 proto-cluster in the soft-band (0.5-2 keV) and hard band (2-10 keV), showing a centrally concentrated $T>10^7$ K (kT > 1 keV) atmosphere within the virial radius for a massive proto-cluster (see Figure 2 below). Once cosmological redshifting is taken into account, such an emission would fall into the observed soft X-ray band, making it detectable in extremely deep exposures. More recently, Bennett et al. (2024, MNRAS, 527, 1033) demonstrated that in a massive, JADES-ID1-like z=6 proto-cluster, virial and feedback-driven shocks heat the ICM to temperatures of $T>10^7$ K (kT > 1 keV), which also results in extended X-ray emission concentrated within the virial radius.

In general terms, in sufficiently massive halos, gravitational collapse alone produces a hot, X-ray-emitting atmosphere largely independent of subgrid feedback prescriptions, which is a generic prediction of models. While cooler gas does extend beyond the virial boundary, its lower temperature and redshifted spectrum render it effectively invisible to current X-ray observatories. This is in excellent agreement with our observational results of JADES-ID1. These same simulations predict a highly disturbed, filamentary morphology, and the rapid accretion of satellite galaxies further perturbs and intermittently shock-heats the ICM, which reinforces that significant high-temperature gas could also be the consequence of massive halo assembly at high redshift. Although a comprehensive review of high-redshift cluster formation theory is beyond the scope of our paper, which is also constrained by its concise format, we

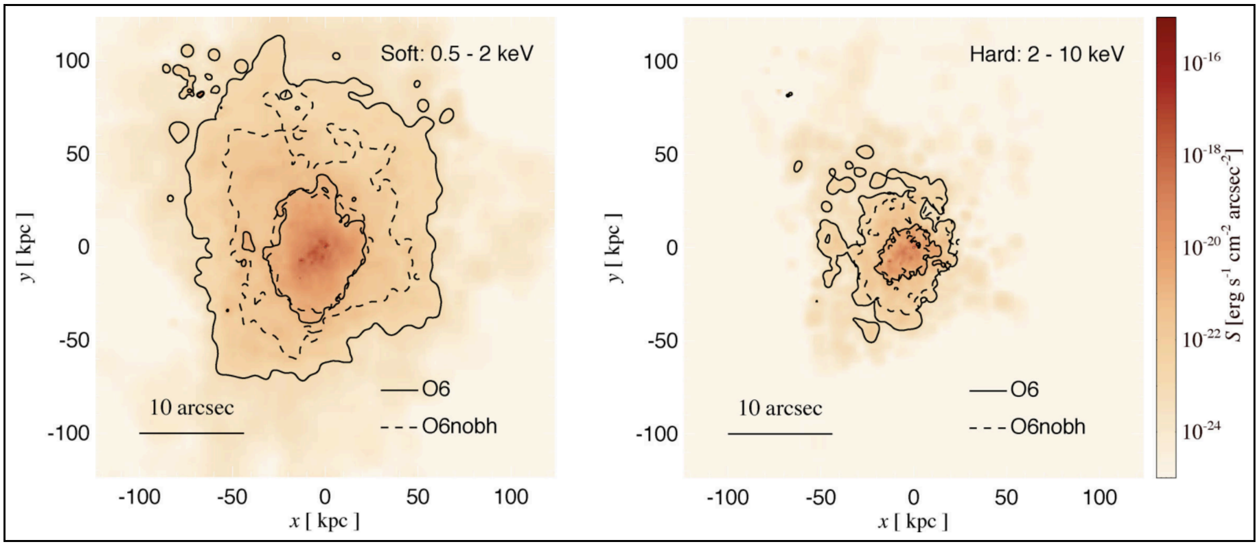


Figure 2: *Simulated X-ray surface-brightness maps of a z=6 proto-cluster with $M_{200}$ = 4.1 x $10^{12}$ $M_{sun}$ from Costa et al. (2014), shown in the soft (0.5-2 keV; left) and hard (2-10 keV; right) bands. The images illustrate both the emergence of a hot (T > $10^7$ K) and X-ray-luminous core within the virial radius and the irregular, filamentary gas structures that extend beyond it. These features are in good agreement with our observations of the JADES-ID1 proto-cluster.*

have added these key references (Costa et al. 2014; Bennett et al. 2024) to highlight the theoretical context.

The authors also report several cases of unrelaxed and merging clusters in which the ICM emission is displaced from the center of the structures, but these structures are generally significantly larger and more massive than the z=5.7 structure, such that the gas can be heated via shocks more easily. In addition, these structures always show a well formed core centered on a beacon (radio, X-ray) and a more irregular morphology in the outer regions.

Explaining the shift between the X-ray emission and the optical center of JADES-ID1 with these examples is not fully justified.

We thank the referee for this important comment, as it has led us to clarify this point in more detail. As discussed in our previous response, shock heating naturally accompanies gravitational collapse once halos exceed a critical mass. Birmboim & Dekel (2003), Keres et al. (2005), and Dekel et al. (2009) show that sufficiently massive dark matter halos invariably develop a hot atmosphere. At ~$10^{13}$ $M_{sun}$ mass, which is the mass we infer for JADES-ID1, a halo reaches a virial temperature of kT ~ 2 keV and because the cosmic mean density was ~300 times higher at z=5.7, the resulting accretion shocks are both stronger and more spatially compact than in low-redshift counterparts of similar mass.

Forming a centrally peaked cool core, in which the ICM brightness maximum coincides with a dominant central galaxy, requires several billion years of relative quiescence, which time was not available at z>5. Cosmological simulations confirm that nascent clusters exhibit clumpy, filamentary hot gas distributions, often offset from any single subhalo, exactly as we observe around JADES-ID1. Unlike relaxed, low-redshift systems, these early halos may not have time to assemble a massive BCG nor to ignite a powerful central AGN. This is in agreement with the fact that we find no evidence for AGN emission in our X-ray analysis (see Section 4 of the Methods). Similarly, previous studies of individual high-redshift galaxies as well as stacking analyses predominantly indicated the X-ray faint nature of galaxies. Thus, the absence of a well-defined core and single luminosity peak is not an argument against association with JADES-ID1, but rather the expected signature of ICM assembly in the first billion years of cosmic history.

Referee #4 (Remarks to the Author):

I co-reviewed this manuscript with one of the reviewers who provided the listed reports.

We again thank all four referees for their careful reviews, which have substantially improved our manuscript. We are very pleased that all four referees recognize the importance of this work and that Referees #2 and #3 recommend publication. Below we address the remaining points raised by Referee #1 who was also positive about the latest revision. We also thank the Editor for considering our paper and hope they agree that JADES-ID1 is an extremely unique object that could lead to a new understanding of how early structure formation assembles in the universe.

Referee #1 (Remarks to the Author):

I would like to thank the authors for the effort they put into updating the manuscript and, in particular, for performing the requested test of the potential foreground contamination. I find the results presented in the paper and in their report convincing.
Although the authors have satisfactorily addressed all the comments raised in the previous review rounds, I have a few minor points that should still be addressed.

- Given the comparison with the simulation works by Costa+2014 and Bennett+2024, presented as part of the response to Referee 3's comments, I do not think the statement that "cluster-scale virialization may have begun sooner than previously thought" is actually meaningful. First, the very concept of "virialization" (as already noted in earlier reviews and based on the broader discussion in the paper) may not hold, and the identification of X-ray emission is simply tracing thermalized gas. Second, if simulations indeed predict the presence of hot gas at temperatures >10^7 K, then the mere detection of X-ray gas cannot suggest a faster evolution than expected from current cosmological and physical models.

We agree with the referee that the term "virialization" was not sufficiently precise in this context. What is physically relevant is whether the large-scale structure has been able to shock heat its gas to X-ray temperatures and not whether it satisfies the definition of virial equilibrium. In other words, unvirialized clusters can still host hot, X-ray-emitting gas. To avoid this ambiguity we have revised the abstract, which now states that "*substantial ICM heating can occur in massive halos as early as z≈5.7.*" This wording makes clear that our result highlights early ICM heating in an extreme object, without implying a systematic change to the typical timing of cluster formation.

- I suggest avoiding references to JADES-ID1 as a potential progenitor of a Coma-like structure, as this reiterates the misconception of a direct link between massive overdensities at high redshift and massive clusters at z = 0. See, for instance, the discussion in Sect. 4.1 of Remus+2023 (https://ui.adsabs.harvard.edu/abs/2023ApJ...950..191R/abstract)

We agree with the referee that the most massive high-redshift proto-clusters do not necessarily evolve into the most massive low-redshift systems, which could also apply to JADES-ID1. Accordingly, we have removed the sentence referring to the Coma cluster and kept a shorter statement that "*JADES-ID1 is a rare example of a proto-cluster caught in the early phases of virial heating*".

- One final test that I think should be added relates to the potential IC origin of the X-ray emission. The authors tested for the expected X-ray signal based on the (offset) radio source identified in the MeerKAT observations of the field. However, it might be beneficial to simulate the expected radio signal under the assumption that the entire X-ray emission originates from inverse Compton scattering from radio galaxies within the overdensity, and then check whether the predicted surface brightness would be detectable in the available radio observations.

If the X-ray flux originated entirely from inverse-Compton (IC) scattering, the required radio flux would be substantially larger than observed. Adopting a typical magnetic field of 4 $\mu$G, we estimate that reproducing the full X-ray flux via IC would require a total flux of 403 ± 130 μJy. Assuming that this flux is spread uniformly over the 20" radius region where the X-ray emission is detected, the corresponding radio surface brightness would be more than an order of magnitude above the MeerKAT detection limit (even for the lower end of the flux range). Specifically, such a signal would produce a >12σ detections in individual MeerKAT beams across the region, which we clearly do not observe.

The radio source detected near the X-ray peak is substantially offset (~11.5” projected distance) and can be clearly associated with the z=1.173 galaxy discussed in the manuscript. The predicted IC contribution from that source is negligible (<1 count in the soft band and about 0.1 counts in the hard band). Even in the unlikely extreme that the entire observed radio flux (~13.3 μJy) originates at z~5.7 rather than z=1.173, the expected IC signal would produce only about 5 X-ray counts in either band, which is far below the observed X-ray emission associated with the JADES-ID1 proto-cluster.

Based on these, an IC origin for the full X-ray emission is strongly disfavored by the radio data. We have revised and streamlined Section 4 of the revised manuscript to present and discuss these calculations.

- The authors included a reference to future X-ray and SZ telescopes and the possibility of using them to detect the signal from extended ICM haloes. However, this remains too vague. If specific forecasts exist in the literature, these should be explicitly cited. Otherwise, I recommend providing at least a simple order-of-magnitude estimate of the expected detectability in order to quantitatively support the statement.

We thank the referee for raising this point. We have updated and streamlined the final paragraph of the main text, and we added explicit references to representative future X-ray forecasts (Athena) and to next-generation SZ experiments. However, we believe that a detailed, quantitative feasibility study of detecting high-redshift proto-clusters is beyond the scope of the present paper for several reasons and would also not fit due to the space constraints imposed by the journal.

The landscape of future facilities and their expected performance continues to evolve (for example, differences between earlier and current Athena configurations, and a wide range of possible outcomes for Lynx-like or AXIS-like X-ray missions in collecting area and angular resolution). Second, planned SZ programs similarly involve changing design trades that strongly affect survey depth and the mass thresholds for single-object detection. Third, detectability of proto-clusters at z>5 sensitively depends on observing strategy (wide shallow surveys versus deep targeted pointings), assumed ICM thermodynamic profiles and gas fractions, and the high-redshift baryonic physics. Exploring this complex parameter space properly would require a dedicated modelling effort and a single detailed paper on the topic.

Given these uncertainties, and the fact that JADES-ID1 is currently a single discovered high-redshift (z>3) X-ray emitting proto-cluster (whose representativeness is unclear), we limit the paper to citing representative forecasts and to noting that future X-ray and next-generation mm facilities offer the most promising routes for robustly detecting and characterizing JADES-ID1-like systems in the future. Finally, we believe that the discovery of JADES-ID1 could even provide an empirical benchmark that could guide the design and observing strategies of such future facilities.

Referee #2 (Remarks to the Author):

I thank the authors for their thoughtful modifications and responses to my comments. I am satisfied that all my comments have been addressed.

We thank the referee for the careful reading and constructive suggestions, which substantially improved our manuscript.

Referee #3 (Remarks to the Author):

The authors have addressed our comments on the previous paper draft, and the results of the additional tests we and the other referees proposed appear to go in right direction. We still consider both the detection significance and the potential physical association with the high-redshift overdensity as interesting but tentative, but we acknowledge that no other tests can further improve the significance of the claimed results or discard them.

We thank the referees for considering our responses and for agreeing that all the tests point to the same conclusion. Namely, that the JWST-identified galaxy overdensity at z~5.7 is associated with a hot, X-ray-emitting ICM originating from the JADES-ID1 proto-cluster. This conclusion is supported by the soft, spatially extended X-ray excess, the absence of a hard band counterpart (consistent with redshifted, shock-heated gas), and the morphology of the emission, which matches expectations from high-resolution simulations. The additional checks (searching for foreground structures; splitting the observations based on different methods) further support that the signal is neither a local fluctuation nor dominated by a few epochs. Taken together, these independent lines of evidence form a coherent, self-consistent picture that JADES-ID1 hosts hot, X-ray-emitting intracluster gas.

Referee #1 (Remarks to the Author):

I would like to thank the authors for addressing my comments and for the improvements made in the revised version of the manuscript. Despite this, I still have some concerns regarding the interpretation of the X-ray and radio data. Specifically, the authors derived constraints on the radio flux under the IC scenario, assuming a "typical" intracluster magnetic field of 4 μG. Such a value is rather extreme even for local galaxy clusters, which are observed to host magnetic fields with an average strength of ~2 μG up to z~1 (e.g., https://ui.adsabs.harvard.edu/abs/2021NatAs...5..268D). Given the strong dependence of the radio power on the assumed magnetic field (and the relevance of this test for the reported detection), it would be important to report how different assumptions about the intracluster magnetic field would affect the derived constraints. Further, to avoid appearing arbitrary, any specific quantitative assumption should be supported by appropriate references to the literature.

We thank the referee for this final comment and are very pleased to clarify this last detail. We agree with the referee that IC constraints depend sensitively on the assumed intracluster magnetic field. Therefore, we have re-examined our calculations and added the relevant citations to support our claims. We also present the reasoning behind our calculations in more detail, for which we include a summary below.

The radial dependence of the magnetic field in galaxy clusters is commonly modeled as following the electron density, $B(r) = B_0\left(\frac{n_e}{n_{e,0}}\right)^{\eta}$, where $\eta$ is motivated by various physical assumptions, such as equipartition between thermal and magnetic energy ($\eta = 0.5$), flux-freezing ($\eta = 2/3$), or a correlation between the X-ray emission and the rotation measure ($\eta = 1$). Observational studies typically find $B_0 \sim 5 - 10\ \mu G$ (Murgia et al. 2004; Liang et al. 2008; Bonafede et al. 2010; Brüggen 2013; Vacca et al. 2021), and use $\eta = 0.5$ or 1.

For the JADES-ID1 proto-cluster at z = 5.68, we estimate a volume-averaged (within the X-ray emitting region) magnetic field using an electron density profile derived from the observed β -model (Fig. 2 in our manuscript), together with the above range of $B_0$, which yields $\overline{B} \approx 2 - 5\ \mu G$. The Di Gennaro et al (2021 Nature Astronomy) reference cited by the referee suggests a “typical” cluster magnetic field of $2\ \mu G$. The same study also finds no redshift evolution of the cluster magnetic field.

After further investigating the literature, we note that the magnetic field in radio galaxies has been reviewed by Croston et al. (2005) for the 3C sample. They found typical fields of $\sim 10\ \mu G$ (with a lower limit of $2\ \mu G$) based on X-ray IC flux measurements, while equipartition estimates often give even higher values.

We emphasize, and the referee also makes clear, that the details on the magnetic field parametrization is not the focus of this work, and our tests show that reasonable variations in $B$ does not affect our conclusions in any significant way. Even for $B = 2\mu G$ a radio flux of

$122 \pm 37$ μJy would be expected in the Meerkat image to explain the observed X-ray emission with IC, which is still 9 times higher than the observed radio flux, including the emission from the offset radio galaxy at z=1.2. Even in the case of an unphysically low magnetic field that produces the observed soft-band (0.3-2 keV) counts, an equal number of hard band (3-7 keV) counts would be expected, which is clearly not observed. These considerations all place limits on the observed X-ray and radio data under various scenarios, which we now summarize in the paper in more detail to address this point.

Finally, I would like to add a minor remark about the closing paragraph of the manuscript. I agree with the authors that a proper assessment of the high-z detection capability of future facilities is well beyond the scope of this paper. However, I would like to stress again the importance of providing the proper literature context for any such statements. The cited references are outdated and mention facilities that have undergone major rescoping or cancellations, undermining the authors' goal of "citing representative forecasts". Although I realize this is only a conclusive statement in the manuscript, I feel like more care should be taken – as the authors also note in their response, especially given the fast-evolving landscape of both X-ray (to name a few beyond AXIS and Lynx, mentioned by the authors: NewAthena, LEM, HUBS) and SZ/CMB (e.g., SPT-3G/3G+, CMB-HD, AtLAST, ALMA WSU) telescopes.

We thank the referee for this suggestion. We have updated the reference list and added more recent citations for the next-generation X-ray and SZ/CMB facilities specifically mentioned by the referee. At the same time, a fully exhaustive list of all evolving mission concepts is not feasible given Nature's formatting guidelines and reference limits. To balance completeness and brevity we therefore included three-three representative, up-to-date references covering the major X-ray and SZ/CMB initiatives.