

Referee report

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Title: Intracluster Medium Entropy Profiles for a Chandra Archival Sample of Galaxy Clusters

Edited V1

This paper presents an analysis of an extremely large number of Chandra archive observations galaxy clusters, and represents the largest homogeneous investigation of the properties of the ICM entropy in the cluster population thus far undertaken. The paper is concerned principally with the properties of the entropy profiles, and more specifically with the distribution of core entropies. The key results of the study are the detection of the presence of a distinct bimodality in the core entropy of the cluster ensemble, and a flattening of the core entropy in the majority of systems, both suggestive of the effects of episodic AGN feedback on the ICM. The bimodality is reproduced both in the full sample and in a (more or less) complete flux limited subsample. The paper and associated data products (available on a separate webpage) represent a valuable contribution to the study of the thermodynamic properties of the intracluster medium, and as such it is certainly deserving of publication.

I do have a few caveats that I'd like to see addressed before recommending publication, along with a list of smaller substantive and stylistic suggestions. I hope the authors take these in a positive light when preparing the revised manuscript.

1) Section 3.4, describes how the values of K_0 were derived and shows in Figure 2 the ratio of the core entropy using the two methods. I count about 40/239 systems for which the ratio is significantly different from unity, which is a non-negligible fraction. While it's pointed out in the text that the different interpolation schemes do not affect the conclusion of non-zero core entropy, I am wondering how the different temperature interpolation schemes affect the bimodality in the K_0 distribution. Since the bimodality is the principal result of the paper, I think the effect of the temperature interpolation on this result should be investigated.

2) Related to this, Section 4, on systematics, is very interesting. However, the entire Section is based on the systematics introduced by angular resolution effects on the density profiles, yet there are clusters in the sample for which there are only three temperature bins. I would like to see some discussion added on the effect of finite binning of temperature profiles. This is especially important because the temperature bins are (rightly) defined according to a total count criterion, meaning that clusters with centrally peaked emission will yield entropy profiles of generally better resolution than those with flat central emission. For instance, what happens when the temperature profile of a cluster with centrally peaked emission is binned to the typical resolution of a cluster with flat central emission?

3) I'm not sure Section 4.2, on the subject of XMM profiles, is entirely correct. Taking the example of the samples of Piffaretti et al (2005) and Pratt et al. (2006), it is important to note significant differences in the respective analyses. My understanding of Piffaretti et al's analysis is that they derived their gas density profiles from spectral fits to relatively coarse annular bins defined for the temperature profile analysis. However, I understand that Pratt et al. derived their density profiles from deprojected, PSF deconvolved analytic model fits to surface brightness profiles, corrected for radial variations in emissivity due to temperature and abundance gradients, and their temperature profiles are also corrected for PSF effects (as described in Pointecouteau et al 2005). Furthermore, Croston et al (2006) show that their analytic surface brightness models recover the density distribution exceptionally well in comparison to Chandra results, with differences occurring only in the single central surface brightness bin. Thus the Pratt et al analysis is undertaken on a radial surface brightness binning scale that is equivalent to that of the present paper, while the Piffaretti et al analysis is not.

Combining a discussion of resolution effects in the context of redshift with a comparison with XMM profiles confuses the issue further. I thus suggest that Section 4.2 should deal only with resolution effects in the context of cluster redshift, and that a separate Section should be added comparing with XMM results. The latter Section will have to discuss the effect of the different analysis methods employed. My belief is that since the XMM profiles were never fitted to anything other than a power law, it is disingenuous to conclude (especially in the case of Pratt et al.) that these results did not find core flattening because of resolution effects. It is more likely because, unlike the present work, they did not think to investigate core flattening.

4) What are the implications of the bimodality? The existence of a gap in the distribution of core entropies implies that either the gap is intrinsic to the cluster population, or that the entropy in the cluster cores is a constantly evolving property. Both of these possibilities open up intriguing areas for further discussion.

If the gap is intrinsic, how is it that some clusters have a high core entropy and some clusters have a low core entropy? The idea suggested by McCarthy and collaborators, that different clusters experience a different level of preheating, may be one explanation. I'm sure the authors could think of others.

If the gap is a function of evolving core entropy distributions, the fraction of clusters on one side or the other, and particularly the evolution of that fraction over time, might be indicative of a characteristic timescale. If that timescale is of the order of Gyr, then it would suggest that dynamical (gravitational) processes are modifying the core entropy, through mixing of high and low entropy gas during mergers. If the timescale is of order Myr, then it is possible that AGN-driven feedback is the dominant driver of the distribution of core entropies. Is it possible to investigate this with the current sample? If not, what kind of sample would be needed to do this kind of thing?

Is there anything special about the clusters in the gap?

I think the paper would benefit from a bit more discussion along these lines added to the text.

5) This may be a little more work, but I think the paper would also benefit from a cross correlation of K0 with the presence of a compact central radio source. Since AGN are thought to be key to feedback in the ICM, such a cross-correlation seems to be the obvious thing to do with such a large and well-observed sample, especially in the context of an ApJS paper.

Other minor issues:

* Sect 1, para 2 (page 2):

"...otherwise tight mass observable relations...", I would say "otherwise theoretically tight".

O'Hara et al. (2006) could be added to the list of references concerned with reduction of scatter about scaling relation.

* Sect 1, para 7 (page 4)

Pratt et al. (2006) could be added to studies showing core entropy has larger dispersion than at large radii.

* Sect 3, para 1 (page 7)

The effect of the choice of center is another source of systematics. Clusters with a large-scale centroid far away from the surface brightness peak will have flatter central entropies, or central entropies that yield physically impossible values on deprojection. Also, within what radius was the centroid defined?

* Sect 3.1, para 8 (page 9)

Regarding deprojection, steeply declining T gradients would have lower central entropy, but increasing T gradients would have higher central entropy.

* Sect 3.2, para 3 (page 10)

The T profile interpolation scheme is not particularly well described, particularly outside the core regions. Was there a model used? How well did this perform for the case of T profiles with 3 radial bins? Also, were the interpolated abundances also used to correct for emissivity variations or was it just the T variations? Abundance variations have more of an effect on emissivity than T variations.

* Sect 3.3, para 1 (page 11)

Quantitatively, how suitable an approximation were the beta models, i.e., in terms of chi squared?

* Sect 5.2, para 5 (page 23)

The fact that the KMM test indicates that two statistically different populations are not present at $z > 0.4$ warrants more discussion. Is this due to resolution effects, or is it an evolutionary effect? If the latter, what

does this tell us?

* Sect 5.5, para 1 (page 26)

I would specify that the Tozzi & Norman results were for semi-analytic models and the Voit et al results were for cosmological simulations with only gravitational effects included.

* Sect 5.6, (page 27)

This section should be revised as per point 3 above.

* Figures:

- Fig4: The legend ($T_x < 4.0$; $8.0 < T_x$) is a bit confusing.

- Fig 4: I suggest addition of two more panels showing the subsamples with $z < 0.4$ and $z > 0.4$