**Decision Letter (MN-10-1576-MJ)**

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**Subject:** MNRAS: MN-10-1576-MJ

**Body:** Dear Dr Cavagnolo

I attach the reviewer's comments on your manuscript entitled "Direct Evidence of Quasar Radiative and Mechanical Feedback in IRAS 09104+4109", ref. MN-10-1576-MJ, which you submitted to Monthly Notices of the Royal Astronomical Society.

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I look forward to receiving your revised manuscript.

Regards,

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Reviewer's Comments:

Dear Editor,

Regarding the paper MN-10-1576-MJ, "Direct Evidence of Quasar Radiative and Mechanical Feedback in IRAS 09104+4109" by Cavagnolo, Kenneth; Donahue, Megan; Voit, M; McNamara, Brian; Sun, Ming.

In this paper, the authors report an X-ray (and radio) study of the unusual system IRAS 09104+4109, an ultraluminous infrared galaxy which is also the brightest central galaxy of a massive, moderate redshift (z=0.44) galaxy cluster. Past studies have found evidence of a heavily absorbed AGN hosted by the galaxy, and this work aims to clarify the interaction between the AGN and the surrounding cluster primarily through a detailed analysis of a deep Chandra dataset. The authors claim to find evidence of asymmetries in the Chandra image, which they attribute to excess emission in the ICM and cavities, the former of which may be a consequence of cool gas displaced by the radio jet, nonthermal emission or beamed quasar radiation interacting with the ICM, while the latter may arise due to gas displacement by the radio lobes. Taken at face value, these features appear to indicate that the AGN is interacting with its cluster environment in both radiative and mechanical ways, allowing the authors to assess the relative importance of these effects in a single system. The authors also consider the spectrum of the AGN, and confirm the conclusions of previous studies that it is consistent with reflected emission from an AGN buried in a Compton-thick medium. The exposition of new, deep Chandra data on this obviously interesting source, and the potentially interesting conclusions that can be drawn (and, indeed, are drawn by the authors) are of sufficient importance to merit the publication of such a paper in Monthly Notices.

That having been said, I have serious misgivings about this particular paper as currently written which make me unwilling to recommend it for publication, at least in its current form. Specifically, the main, new results of this paper are based almost entirely on the visual identification of purported features in a processed image of the system (the aforementioned ICM excesses and cavities), which are boldly stated to be "unambiguously" detected, without any evidence presented as to the robustness of the "detections" (statistical or otherwise). The authors' vague talk elsewhere of spectral models being "preferred", without quantifying what this means is, similarly, symptomatic of a general lack of preciseness in this work. Before I would consider this paper to be suitable for publication, these issues will need to be addressed, as described in more detail below.

The comments below are not exhaustive; in the interests of preventing this (already lengthy) report becoming too bogged down in minor technical or stylistic details (that may be addressed anyway in a revised paper), in this initial report I focus only on the most serious issues affecting the Chandra data analysis and the clarity of the paper.

Major Issues
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Image analysis
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The morphological analysis of the authors, outlined in Section 7, is central to the new results presented in this work. To identify structures, the authors fitted a smoothed elliptical isophote model to the image, and residuals from this model were interpreted as cavities and an ICM "excess". Unfortunately, a decrement or an excess over a particular model is only meaningful if that model is itself physically meaningful, and in

the present work, the authors provide no evidence to support the physicality of the particular model which is fitted. By inspection of the residual image (Fig 5), what is immediately apparent is that the excess is oriented roughly east-west, while the decrements are oriented in an almost exactly perpendicular direction. This is exactly what is expected when fitting a model which does not have the correct shape. (An example would be fitting such a model to an image which has a single excess, in which case the normalization, and ellipse orientation would likely be distorted by trying to fit this feature, producing ostensible deficits elsewhere).
In light of this, I am left deeply skeptical of the authors' interpretations of these features.

Nevertheless, I am prepared to accept that the image is asymmetric, as suggested by Fig 1 (right panel; although the absence of a colour-scale for this image, and the use of smoothing, makes it hard to assess the formal statistical significance of the asymmetry). Morphological disturbances are actually fairly commonplace in the centres of normal galaxy clusters (which, according to the authors, this is) due to, for example, "cold fronts", cavities (such as are postulated here), or (possibly) shocks. Since the nature of the asymmetries is essential for the results in this paper, the problem for the authors is to establish that the only viable explanation for the morphology of the gas is an excess in the east-west direction, and cavities perpendicular to it. Could the morphology be explained in terms of: only excess(es), only cavities, or possibly disturbances similar to cold fronts? The question of cold fronts, in particular, is summarily dismissed by the authors in Section 4, where they argue that there is an absence of evidence of temperature or density discontinuities in the radial profiles, and in a 2D analysis. However: a) the 2D analysis is neither explained, nor the results of that analysis presented in this paper (hence the text discussing it should either be removed, or considerably expanded, and figures presented). b) the absence of evidence is not evidence of absence. Are the data also consistent with the asymmetries being (at least in part) due to cold fronts?

Next, even if we take at face value the residual plot shown in Fig 5, there remains the question of the statistical significance of the features which are shown. I appreciate that there is something of an industry of visually identifying and interpreting features in galaxy cluster images, without quantifying the formal statistical significance of those features. This is, in my opinion, only acceptable insofar as the S/N is high enough for features to be unambiguously evident in minimally processed images, so that the computation of a formal significance is somewhat redundant. This is self-evidently not the case in this paper; the S/N appears low and the "cavities" are only visually identified by apparently stretching the colour-scale on a residual image. Coupled with the scientific importance of finding clear cavities in a system such as IRAS 09104+4109, an "unambiguous" detection demands a more thorough, quantitative analysis. There are a number of ways this could be done, but I would suggest at minimum a Monte-Carlo analysis, in which realistic images are generated from a smooth, elliptical model, but with exesses similar to the purported features (but no cavities). These should be processed identically to the real data, and any spurious cavities assessed and characterized. Since the authors are using visual inspection to identify cavities, for this to be done objectively, a clear set of criteria will need to be adopted to explain what constitutes a cavity, and how its size and position are determined. A similar kind of analysis could be carried out to investigate the excess(es).

The Derived Jet Power (Sect 7.2)
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The derived jet mechanical power is obtained from the properties of the cavities that are identified by the authors. However, the error-bars are determined entirely from the uncertainties on the spectroscopic properties of the ambient gas (eg pressure, sound speed), rather than the position and geometry of the cavities. I suspect this latter to be a major source of uncertainty, and so it will need to be folded in. Perhaps the authors could achieve this by Monte-Carlo simulations similar to those outlined above, in which cavities are added to the model before simulation, and the errors on the geometry and location are assessed. (This could also allow the authors to place limits on as yet undetected cavities, that could affect the jet power estimation).

The Excess Spectra (Sect 6)
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The discussion of the analysis in this section was lacking in some necessary detail. How are the different regions chosen and characterized? In particular, where are the background regions? Performing a background subtraction, such as described, makes sense only if the "source" region is expected to have the same spectrum as the "background", with an additional component added. If it is actually hot gas at a different temperature, what you would obtain by doing a subtraction is unclear. Since the authors already have background spectral models for everywhere on the CCD (used in Sect 4), why don't they employ these and fit the data in the source and background regions separately? This would considerably aid in interpreting these data, and assessing whether we're just dealing with temperature asymmetries. I'm assuming the fits shown in Table 3 are "background subtracted"; which background was used for the "EEx bgd"?

The authors discuss some models as being a "poor fit". What does this mean; How was it assessed; How poor was the fit? Some models are apparently "preferred" over others; apparently Monte Carlo simulations were used to ascertain this, but these are not explained, nor the significance of the difference quantified. Additionally, what is the impact of varying the metallicity? Figures would be especially helpful here: The authors should overlay on Fig 6 (right panel) the best-fitting model without the additional Gaussian lines. They should also decompose their preferred model into its different components: the ICM component, and the Gaussian lines, and provide a comparison between this parameterization and the cloudy model they adopt.

Less serious issues
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The ordering of the material was confusing. Section 3 uses results from Section 4; Section 6 uses results from Section 7. I could see no reason why these respective sections could not be re-ordered to aid with clarity.

Section 3: I was very puzzled at this section, which seemed rather unconnected to anything else in the paper. I can understand the authors wishing to assess whether the cluster is "ordinary" by asking if it obeys standard scaling relations, and in that sense some of the material here is relevant. However, this is never done in this section; rather they compute the gravitating mass and gas mass within R_200 by extrapolating fits within 0.2 R_200 to the temperature and density profile, while the global parameters from the scaling relations are computed within R_500! They at no point explain why we should be interested in the tedious tabulation of various global parameters given in Table 1, and don't use them elsewhere in the paper. Therefore, it was unclear how this material led the authors to the conclusion "RX J0913.7+4056 appears to be a typical massive, relaxed galaxy cluster."

Furthermore, there is evidence that a beta-model is not a good global description of the density profile of galaxy clusters when fitted over a large radial range. The authors state "the gravitating and gas mass calculations include significant extrapolation, and thus may be lower limits, athough their ratio should not be significantly different", but I did not understand this argument. Surely, if the gas density profile steepens at large radii (eg Vikhlinin et al, 2006), the gas mass would be over-estimated by their extrapolated fits? Why is the gas mass/ gravitating mass ratio robust? A more massive system will tend to have a steeper gas density profile (from the equation of hydrostatic equilibrium), and hence if the gas mass is over-estimated, I'd expect the total mass to be underestimated.

Section 4: The deprojection method for computing the gas density needs to be summarized, in addition to the citation to Cavagnolo 2009. Having read the description in Cavagnolo 2009, I am still puzzled as to how the emissivity is incorporated self-consistently into this calculation. Surely \eta (r) is determined from the projected temperature/ abundance profiles, so doesn't this assume that the projected and deprojected temperature/ abundance profiles are identical? How do errors on the metallicity/ abundance profile propagate into \eta, and hence onto the gas density profile? How are the errors on the gas density computed? The authors should provide details of how the Monte Carlo was performed.

The quality of the fit to the entropy profile is not discussed. The best-fitting model should be overlaid on Fig 3. If the system is morphologically disturbed in the core, that means that the deprojection which is implicit in the derivation of the entropy is not going to be correct, and the gas entropy within, say, 30 kpc, will not be correctly derived. How much will this affect the fit presented in this section?

Table 2: Please define (not just provide units for) all parameters that are listed.

Figure 5: Please show and label the cavities, as well as the "excess" regions. Please include labels for each specific excess region.

**Date Sent:** 21-Sep-2010