Decision Letter (MN-10-1576-MJ.R1)

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Subject: MNRAS: MN-10-1576-MJ.R1

Body: Dear Dr Cavagnolo

I attach the reviewer's comments on your revised manuscript entitled "Direct Evidence of Mechanical and Radiative Feedback from the Quasar in IRAS 09104+4109", ref. MN-10-1576-MJ.R1, which you submitted to Monthly Notices of the Royal Astronomical Society.

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I look forward to receiving your revised manuscript.

Regards,

Fern

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Reviewer's Comments:

Many apologies for my tardy response. I have now assessed the revised manuscript, MN-10-1576-MJ.R1, "Direct Evidence of Quasar Radiative and Mechanical Feedback in IRAS 09104+4109" by Cavagnolo, Kenneth; Donahue, Megan; Voit, M; McNamara, Brian; Sun, Ming, and am pleased that the authors have clearly taken my previous report very seriously in redrafting the paper. While I was encouraged to see that the paper clarity was substantially improved, the new version of the paper does, unfortunately, raise some worrying issues that make me even less convinced as to the existence of the purported cavities, and more convinced that they are over-interpreting systematic and statistical noise in their images. This is a very serious issue, and if the authors cannot, in their next revision, convincingly demonstrate the existence of cavities that correlate well with the radio emission, I believe that the paper will need to be substantially modified to remove any claims of such a detection. I outline below my concerns.

Existence of the Cavities
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The updated analysis raises some very serious questions as to whether the alleged cavities are real or, as I suspect, the authors are misinterpreting noise as science. Figure 3 is a useful exploration of the sensitivity of the results to the underlying surface brightness model which is subtracted off. As I pointed out previously, deficits or excesses over an arbitrary model are meaningless unless the model is physically justified. In the present case, the models fitted are very simple (more of this later), but even fitting them, what is abundantly clear is that the morphology and the amplitude of the alleged cavities (as well as the purported excesses) are strong functions of the underlying model subtracted. In the most realistic case, Model B, the isophotal ellipticity and position angle is allowed to vary with radius, but this is still overly-simplistic as there is, a priori, no reason to believe that a galaxy cluster without cavities is always well described by such a model. Still, even with these simplifications, the cavities have all but disappeared when this model is used.

In their reply to my last report, the authors emphasize: "an essential component in claiming the cavities and excesses are real structures is their one-to-one correlation with emission features across multiple wavebands". I agree with this statement, so let us consider how it applies in this case (model B). There is approximately a 180 degree deficit surrounding the nuclear region. Such features are commonly seen if the fitted model does not adequately describe the very peaked (asymmetric?) central surface brightness region. This is especially true of an "unsharp masked" analysis (Fig 2). There is also a strong deficit to the east of the nucleus that is unassociated with the radio jet, but which appears stronger than the purported cavities to the south east or upper north west. Additionally, there is a deficit, as strong as the alleged south-eastern cavities, to the south west that is also unrelated to the radio emission, and a further depression significantly eastwards of the southern "cavities". Taken together, it appears there is actually quite a poor correlation between the radio features and the alleged cavities. Even allowing for the fact that cavities and radio emission do not typically match closely, the morphological disparities are so large as to cast serious doubt on there being a "one-to-one correlation" between the radio jet and these depressions, unless the authors start picking and choosing which of the "cavity-like" features they interpret as real, which is formally incorrect. Unfortunately, it appears that is exactly what they are doing here; in Section 3, they state: "There are additional deficits east of the eastern excesses and south of the WEx which lie along off the nuclear beaming axis and are further from the core than the detected cavities. The

extra decrements are unassociated with any radio emission and their physical significance is unclear."

To determine the statistical significance of the purported cavities, the authors adopt two different approaches, which they dub Method 1 and Method 2. As I understand it, Method 2 is essentially computing the surface brightness (SB) averaged over an arbitrary radial range in a number of sectors, and comparing the SB in the vicinity of cavities with the azimuthally-averaged mean. This has very limited usefulness, since, unless the isophotes are exactly circular, there will inevitably be statistically significant variation with position angle. Given the orientation of the model isophotes in Fig 3, upper central panel, it would be unsuprising if the SB appears lower than average in the south eastern and north western regions (as reported), but solely because of the elliptical morphology of the gas. Method 1 is potentially more useful, as it involves comparing the SB of the data with the fitted surface brightness model. What appears to be done is that the authors are computing the mean SB in arbitrary regions drawn around each feature of interest, in both the data and the model, and computing the ratio. Unfortunately, the interpretation of these ratios in terms of a "detection significance" is nontrivial, since one has to take into account the "number of trials" carried out in searching for cavities since, a priori, one does not know exactly their location. (For a discussion of the critical importance of this issue, albeit in the context of spectroscopy, see Kaastra et al, ApJ 652, 189.) Failure to account for this factor can lead to spurious high-significance "detections". In the present, brief paragraph on these methods (and, so far as I could tell, in the Wise paper, to which the reader is referred for a "similar" approach), it is not adequately explained if (and how) the authors have taken into account the "number of trials" in assessing the statistical significance.

In the authors' reply to my last report, they argue "We disagree that our charge is to establish that only excesses and cavities can reproduce the observed ICM morphology". I find it hard to understand this statement, since, so far as I can tell, that is exactly what they are claiming in this paper. They do accept that other factors, such as "cold fronts" could be responsible, but argue that, since they cannot definitively detect such features (due to S/N), they cannot claim to detect them. While I agree that they cannot confirm cold fronts or shocks, I strongly disagree with their position that the "simplest" hypothesis is the highly unusual detection of an AGN interacting both mechanically and radiatively with the cluster; this is particularly a concern in light of my misgivings about their "cavity" identification. If the authors cannot distinguish between two hypotheses, namely-- a) the morphological disturbances are due to cavities and excesses and b) the morphological disturbances are due to cold fronts or shocks, then this should clearly be stated in the abstract, discussion and conclusions summary.

The excesses
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In their reply to my last report, the authors state "Utilizing the deep backgrounds for spectral analysis does not address the nature of the excesses because cluster emission will not be removed." This does not address my comment that: "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." That is, in my opinion, the authors are making an assumption that they have not justified, ie that the only interpretation of the "excesses" is a source of emission on top of the cluster emission. An alternative explanation is that they represent cluster emission that is slightly higher density (and, assuming pressure equilibrium) at a different temperature than the surroundings. I see no reason in the current text to reject this hypothesis, so the authors should discuss *both* cases.

Minor issues
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In Sect 7.1, the authors still don't clearly state whether the cluster obeys standard scaling relations or not which is, I suspect, what they're trying to do here.

Typo, page 2, line 31: "XXX" should be something else?

Sect 3, para 2: The text claims that they "adaptively smooth with a 1" Gaussian", which seems a contradiction. Do they use adapative smoothing (in which case the analysis would be suspect), or Gaussian smoothing with a fixed kernel?

Sect 4.2 10% uncertainty to account for projection effects seems entirely arbitary; where does this come from? The authors should justify the magnitude.

Date Sent: 04-Mar-2011