

Mapping the hot gas temperature in galaxy clusters using X-ray and Sunyaev-Zel'dovich imaging – First referee report

Dear Referee,

Thank you for your comments and corrections, which will improve the quality of the paper. Please find the answers and modifications we have done listed below. The changes in the text have been made in pink.

The letter by Adam et al. reports a two-dimensional comparison between gas temperatures of MACS0717 measured using the classical X-ray spectroscopic technique and a method combining the gas density measured from X-rays with the high-resolution SZ map measured by the NIKA instrument. While this is indeed a nice attempt to apply a novel technique in 2D, I really don't see what justifies publication of the paper in Letter format. Indeed, the content of the paper does not appear to be particularly pressing, both from the point of view of the presented data and of the (relatively modest) scientific results. However, I would be happy to recommend publication as a normal paper once the following issues are addressed:

We follow your recommendation and resubmit this paper as a normal paper, after including your comments.

1) As is correctly mentioned in the introduction, the gas densities measured in X-rays are affected by gas clumping (see e.g. Nagai & Lau 2011, Vazza et al. 2013). This affects the conversion between emission measure and line-of-sight density integral (the l_{eff} parameter in Eq. 4). Taking clumping into account is already difficult enough in relaxed clusters, however in a system as chaotic as MACS 0717 it is essentially impossible, especially in 2D. The simple models M1 through M3 described in Sect. 3.2 somehow attempt at modeling the geometry of the cluster, however the morphology of MACS 0717 as revealed by Chandra is far more complicated than any of these toy models, and it is reasonable to imagine that the method proposed here largely underestimates the effect of clumping.

This is very important, as clumping would tend to bias the measured gas densities high and thus to lower the temperatures measured with the X/SZ technique. This could mimic a better agreement with XMM than with Chandra. Please discuss this effect. For a discussion of the amplitude of this effect see Tchernin et al. 2016.

We now discuss further the effect of clumping in the results/conclusion sections.

By the way, while this may be a first attempt at comparing X/SZ and spectroscopic X-ray temperatures in 2D, several works have tackled this issue in 1D already. Recent studies combining X-ray and SZ data to measure radial temperature profiles are not acknowledged. See for instance Nord et al. 2009, Basu et al. 2010, Morandi et al. 2012, Eckert et al. 2013a,b.

Indeed, such attempt was done in 1D and we now include some references. In fact, it was also done previously by our team (Adam et al. 2015, Adam et al. 2016, Ruppin 2016).

2) It is problematic that the method used for reconstructing temperature profiles from X-ray spectra does not include any error bar. The mentioned « conservative » uniform uncertainties of 14% and 29% are arbitrary and incorrect, as I expect that the actual uncertainties should be smaller in the core and larger in the outskirts. Moreover, a temperature of 25 keV is well

outside the fitting range for Chandra and XMM, which would not be the case in 10 keV regions, so there is no reason why the relative uncertainty should be the same in both cases.

Getting X-ray errors has required a lot of development, but it is now implemented and discussed in the text.

3) Similarly, it is just mentioned that the fit to the data in Fig. 3 was made with a χ^2 technique, while in practice the data have considerable uncertainties on both axes. A method taking into account the errors on both axes (e.g. BCES or linmix_err) should be used. Errors on the fitted parameters in Table 1 should be provided such that the reader can grasp the significance of the differences between XMM, CXO and gmw. Finally, the calculated scatter should be the intrinsic scatter, not the statistical scatter. Once again see BCES or linmix_err.

This point is related to the previous one, and we now include X-ray errors in the fit. Nevertheless, while it is a linear fit, the fitted function is not a straight line since the zero level offset is not a constant (it depends on the density). Therefore, we have developed our own fitting method based on existing work and validated it using Monte Carlo. This is discussed in the appendix B.

We also provide errors on the fitted parameters including the full posterior likelihood. They were negligible compared to systematics when including only y-axis errors, but it is not the case anymore. The best-fit cross-calibration values are now slightly higher when including error bars on the y-axis.

The scatter is now computed as the intrinsic scatter.

4) It is commonly accepted (see Mazzotta et al. 2004) that spectroscopic X-ray temperatures tend to underestimate temperatures when dealing with multiphase plasma, as is correctly mentioned in the introduction. For this reason one would expect the gas-mass-weighted temperature to exceed the spectroscopic X-ray temperature, which is not observed here, even when comparing with the (lower) XMM temperatures. Please provide a discussion of the potential reasons for this unexpected result.

A discussion is now provided about this effect.

Overall, it seems to me that the systematic uncertainties (clumping, kSZ, zero-point, spectroscopic vs gas-mass-weighted temperatures, etc.) are at least at the same level as the difference between Chandra and XMM temperatures. Thus I am not sure that the results presented here can actually be used to favor XMM temperatures over Chandra.

This is perfectly true and it was not the point of the paper. We have included further discussion in the introduction, discussions and conclusions to emphasize this point.