Referee report and response

Dear referee,
Thank you for your comments and corrections which will improve the quality of this work. Please find below the answer to your report. The changes in the text have been done in bold face.
Best regards.
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This paper presents constraints on the intra-cluster medium of MACS J1423 using a joint analysis of ground-based SZ data from NIKA and X-ray satellite data from Chandra and XMM. In addition, other external datasets are included to subtract the unwanted signal from unresolved galaxies in the SZ data. The data reduction and analysis methods add to the quickly growing body of literature related to joint SZ/X-ray cluster analyses, and are worthy of publication after addressing the mostly minor comments listed below. Other than noted below, the analysis methods and assumptions are clearly described, new results are emphasized, and the figures and tables are well laid out.

Major comments:

- Section 2.3 states that the noise model does not contain any astrophysical sources of noise (e.g., CMB fluctuations, fluctuations due to SMGs below the Herschel detection limit, etc.) Perhaps these noise sources are negligible in this analysis, but they should be described and quantified.

We have estimated the different possible foregrounds using Planck data and simulations and we discuss them in a new section (2.4).

Minor comments:

- In Section 1 previous SZ pressure profile results are mentioned. A reference to Plagge et al. (ApJ, 716, 1118 (2010)) should be added to the other references. Also, I don't think the claim that existing results are limited to the local universe is valid.

The reference Plagge et al. (2010) is now included. The sentence has been rephrased to avoid confusion. We only mean that even if some results exist at high z, the SZ pressure profile has not been deeply investigated compared to low z, due to the high angular resolution required to probe the profile over a wide range of scales.

- Although the results in this paper are described to be a pathfinder for NIKA2, several locations in the text read as if they are part of a proposal or an advertisement for NIKA2. In particular, the paragraph devoted to NIKA2 in Section 1 and the final paragraph of Section 6 should be toned down (or, even better, moved to the conclusions section under a heading of "future prospects" or something similar).

The NIKA2 paragraph is moved to the end and the last paragraph of section 6 has been toned down.

- I cannot find the noise RMS/beam in the NIKA data given anywhere in the paper. It can probably be inferred from the results in Table 3, but it would be good to explicitly provide the numbers in Section 2.3.

Accounted for.

- In Section 3.2 the criteria for source detection in the Herschel data is never explained. For example, is the source required to be above a given S/N in one Herschel band? Or above a given S/N in at least 2 Herschel bands?

Accounted for.

- In Section 3.2, the technique for subtracting SMGs using Herschel data appears to closely match the technique used in Sayers et al. (ApJ, 778, 52 (2013)), yet no comparison is made to that work.

Accounted for.

- In Section 3.2 the issue of SZ contamination of the source detections at larger radii is not discussed. In particular, Figure 7 indicates a fairly significant non-zero positive SZ signal towards the edges of the 150 GHz map, which I assume would bias the SMG fits to sources at those radii.

The SZ signal does not affect the results at large radii because we are also fitting for a local background around the sources. A bias from SZ is expected only if strong SZ structures (at scales comparable to the beam) are located near the sources, which is not the case at large radii. The fact that the zero level of the map is slightly positive (Fig. 7) is because we have no way to measure it with NIKA only. Therefore, at first order, the mean of the map is zero, such that the external part of the map is positive due to the negative SZ signal at the center. This is taken into account as a nuisance parameter in the analysis and the constraint from the Planck integrated signal helps to define it.

A comment has been added concerning the SZ contamination.

- At the end of Section 3.2 the authors note a lack of sources in a ring around the cluster. This appears to be consistent with what was found by Zemcov et al. (ApJL, 769, L31 (2013)), and so a comparison to that work would be useful.

The two analysis are not directly comparable because Zemcov et al. (2013) have done this measurement after removing the detected sources. Nevertheless, we mention their result in the text. This reference will certainly be useful for future deeper or stacked analysis.

- In Section 5 the authors make the claim that NIKA allows them to break the confusion in the low frequency SPIRE bands. However, given that none of the sources are detected at high significance in the NIKA data, this claim does not seem valid.

Indeed, this is only for detected sources of course. Accounted for.

- The mass model used in Figure 4 should be updated to the more recent model from Zitrin et al. (ApJ, 801, 44 (2015)).

The last version of the model is now used.

- In Figure 3, the strict pixelization of the SPIRE images is inconsistent with the smooth interpolation used for the other images. It would be best to make all of the images in a consistent way.

All the Herschel maps have been reprojected to the same pixel size as the NIKA maps.

- In Section 6.1, how is the well known temperature calibration difference between XMM and Chandra accounted for in the analysis?

The temperature is only a second order effect when extracting the density profile (discussed in section 6.1). The change we obtain on the density profile when using Chandra temperature instead of XMM is less than 1% over the entire radial range. Therefore it does not affect the combination of the SZ pressure to the Xray density and it is not accounted for. On the other hand, when reconstructing the pressure (or the temperature itself) based on Xray data only, the profiles directly depend on the calibration difference (blue data point versus purple data points in Fig.8). In the case of MACSJ1424, we find a consistent temperature between Chandra and XMM. The XMM temperature is even higher than that of Chandra unlike what is generally observed. This is discussed in section 6.3.2 in the paragraph starting with "The constraints on temperature and entropy ...".

- For equation 5, are all of the parameters varied when fitting to the X-ray data? If so, then this should be stated in the text in Section 6.2.

All the parameters are fitted. Accounted for.

- The text in the first paragraph of Section 6.3 appears to state that only the XMM data were used in the fits, and the Chandra data were only included as a cross-check. Is that correct?

Yes it is. When combining SZ pressure with Xray density, the error is completely dominated by the pressure and so the difference between XMM and Chandra is insignificant.

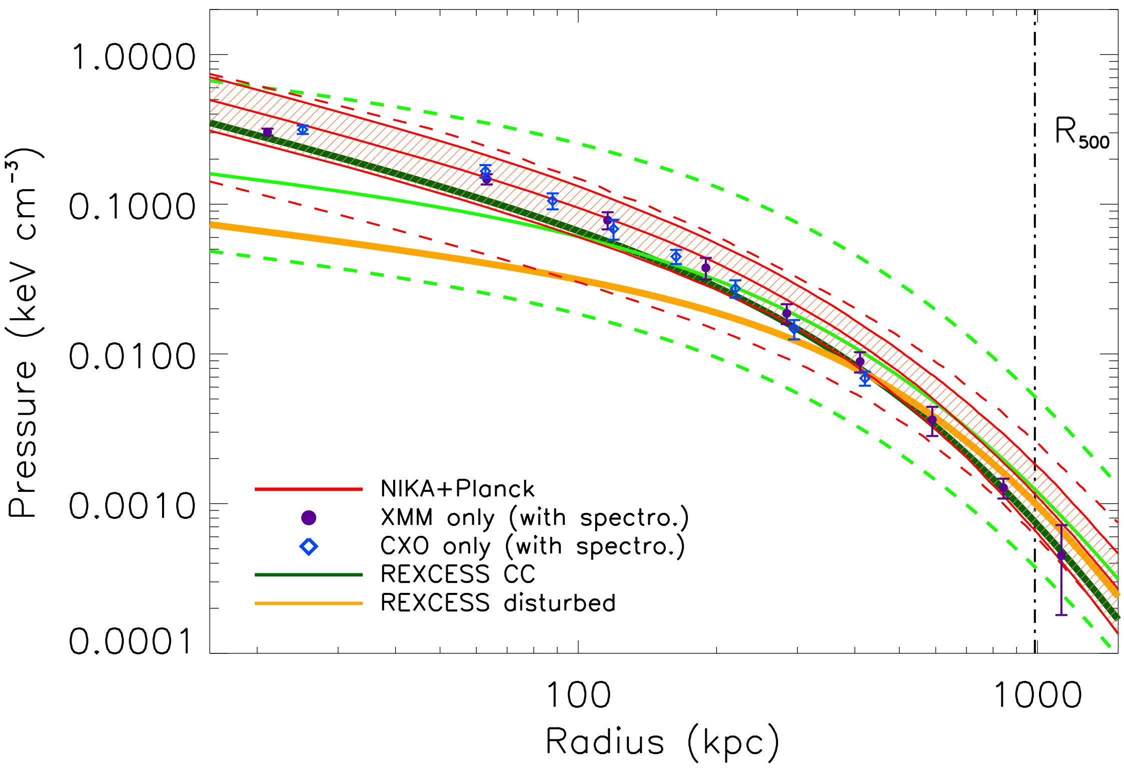
- In Section 6.3 the authors seem to bounce back and forth between using M2 and M3 as the default model for comparison. It would be better if a single model was used uniformly throughout the discussion in this section.

In general M3 is used as our baseline because it is the most conservative model. However, in figure 8, we prefer to use M2 as the dashed model because M3 encompasses M2. In this case, precisions on the limits has been added in the text.

- In Section 6.3.2 the authors' claim a significant improvement in the uncertainty of their fit compared to Bonamente et al. However, I don't understand how such a comparison was made, and the difference in uncertainties is never quantified. It would also be useful to plot the Bonamente et al. fit with the authors' results (for example, in the upper left panel of Figure 8).

Below is the comparison to the Bonamente et al. constraint, provided as light green lines. To do so, we used their Arnaud et al. (2010) fitting parameters given in their table 5. They are fitting P_0 and r_p, fixing a,b,c. They provide uncertainties on both r_p and P_0, but since we know that these two parameters are highly degenerated, we fix r_p to the best fit value and consider only the uncertainty on P_0 to compute the upper and lower limit given in the

figure below (dashed line). This certainly underestimates their uncertainty but considering both P_0 and r_p would overestimate their uncertainty. The mean ratio between their one sigma uncertainty over our 1 sigma uncertainty is 4.2.
A discussion on the improvement of the uncertainties has been added but we do not want to add their constraint to our figure 8 because it will add some confusion to a figure that already contains a lot of information.



- In the left hand panel of Figure 7 it would be useful to change the x-axis from arcsec to kpc for consistency with the right hand panel and also with Figure 8. In the right hand panel of Figure 7 it would be useful to show, e.g., the ratio of the data to the model, as it's impossible to see how well the X-ray data agree given the large spread on the y-axis relative to the uncertainties.

Accounted for.

- In Section 7.2 I find the authors' claim that SMGs will not be a problem because they are detected at 260 GHz to be quite dubious for several reasons, including: the text in Section 3.2 notes that the S/N to SMGs is similar at 150 and 260 GHz, and so the 260 GHz data will have little if any sensitivity advantage over the 150 GHz data; the 260 GHz data will contain SZ signal that must be separated from the SMGs; without additional spectral channels, a very restricted SED will need to be assumed to extrapolate between 260 and 150 GHz; and the steep SMG luminosity function means there will always be a large number of sources just below the detection limit. Without extremely deep and high resolution data capable of resolving the bulk of the SMG population, I think SMGs are likely to be the dominant limiting factor to future SZ observations similar to those envisaged for NIKA2. In contrast, I think the authors are overstating the importance of radio galaxies at the NIKA wavelengths. Although the BCG can be quite bright, the relatively flat radio galaxy luminosity function means that any other contamination will be minimal compared to SMGs.

We agree and the paragraph has been changed accordingly.
However, we stress the fact that the location of the contaminant within the cluster is an important factor. As radio contaminants are often spatially correlated with the Xray/SZ center, their presence direclty impacts any constraint on the cluster core. Moreover, the presence of AGNs is correlated with the clusters dynamical state, which might even bias preferentially a given cluster population. This is why we are very concerned about AGN for future NIKA observations.
In the case of 260 GHz IR source detection with no higher frequency information, it is possible to simultaneously fit for the source and the SZ signal. In fact this was done in Adam et al. (2015) in CLJ1227 and we show the 260 and 150 GHz maps below as an example. Due to SZ signal, the source identification is much more easy at 260 GHz compared to 150 GHz. It can then be marginalized in a SZ+point source simultaneous fit. The source can also simply be masked without affecting the pressure reconstruction if it is far enough from the cluster center. The undetected source are of course a different story, as now discussed in the text.