**Mapping the hot gas temperature in galaxy clusters using Xray and Sunyaev-Zel’dovich imaging**

**Juan referee report**

**26/08/2016**

General comments -----------------

I find the paper is pretty good although I think we should be more careful in the presentation and quantification of the results as described in the following comments.

0) Abstract:

- Add uncertainties when discussing the comparison of the different temperature maps

ok

 1) Introduction

- "In turn, such analysis provides"

ok

- "spectroscopy temperatures are driven by the colder"

ok

- when discussing the simulation it is not clear to me what the given references support exactly and how much the details of the physics change the results

References provide example of studies on the temperature using numerical simulations. What do you mean exactly?

Concerning the details of the effects, it is complicated to give a single number as it depends on many things (including radius, temperature, etc), the reader should look at the references, we cannot add too much text in this letter.

- Add reference when first introcing SZ "The thermal Sunyaev-Zel'dovich effect ...."

ok

- Add reference when discussing MACS J0717 tSZ maps [Adam et al. Sayers et al]  "...... tSZ are currently available."

Ok for NIKA but Bolocam is not resolved enough and MUSTANG data are too much filtered/not sensitive enough, so they would not be appropriate

2) Data

- Adam, 2015, 2016,b,a ->  Adam, 2015, 2016,a,b

This is because the kSZ paper is not published yet, but it will change eventually

- Add reference for NIKA calibration uncertainty summary "...... and stability of the instrument"

Ok

3) Method -> Temperature reconstruction ?

Ok

- "Note that the nature of the algorithm does not allow a direct calculation of the temperature map uncertainties"; fine but can you compute them indirectly?. can you kind of estimate them somehow ? Contribution from statistical noise or systematics, filtering in the process ? I think this needs to be quantified.

Yes. See the full discussion we had by emails. In the end, we estimate the errors to be ~13% using Jack-Knife, even if it can only be an order of magnitude (but upper limit) estimate

 - Equation 1. I think that we should write it as an approximation

I don’t think so. The first part of the equation is exact. The second part is also exact under the ideal gas law condition as specified in the text. Their is no assumption of constant T along l.o.s., if this is what you mean, because it is \bar{T}\_{gmw} and not T\_e. It is the definition of the gas mass weighted temperature in some sense.

- The last comment before 3.2 is not clear to me. I understand that lambda(Te,z) varies little with temperature and that the uncertainties induced by this might small, but I do not understand why this is related to the systematics in the data. Sorry but I am not an Xray expert as you know.

**From Monique:”** C est lie a la conversion entre SX (mesure) et densité (ce qui nous intéresse) : ne^2 \propto SX/Lambda(T). Donc une systématique sur Te en principe se propage en systématique sur Lambda donc sur n^2**”**

- I do not understand why we should define n\_e effective and P\_e effective. Equation 6 could be also be written as Kb Tgmw =\int Pe dl /\int  estimate(ne) dl without including the 1.0/leff factor.

True, but then it would not be a pressure & density anymore. We could also plot the maps in keV/cm-3 Mpc and cm-3 Mpc. This is just to have real density and pressure maps along the line of sight.

- Indicate what it is assumed for the temperature to compute lambda(Te,Z) in Model 2

I don’t understand what you mean. We use the n\_e deprojected profile from the standard pipeline (similar to all the other NIKA papers). We now cite Croston et al. 2006 paper.

- I do not understand the comment about model M3b) "where all the subclusters are located at the same line-of-sight coordinate, i.e. the same redshift".   To be honest I do not understand what it is exactly the assumption your are taking here. Furthermore, it is clear from table 1 that this model is problematic and very far away from the others. This needs to be clarified.  I am not advocating for removing it from the paper but to better understand what it means.

M3 says that each sub-cluster is given by a beta model, i.e., a model for n\_e. To compute l\_eff you need to compute n\_e^2. From M3 alone, you can compute n\_e^2 for individual sub-clusters alone, but you also need to compute the cross term n\_e\_subcluster1 x n\_e\_subcluster2. Therefore you need to know the distance between the clumps to compute cross terms. Therefore, you need to assume a l.o.s. distance between the sub-clusters. M3a assumes that they are far from each other. M3b assumes that they are as close as possible from each other because their l.o.s. relative distance is 0.

The precision that M3b corresponds to the minimal distance between sub-clusters was added. And the reader can always look at the kSZ paper where this is discussed in more details.

M3b is not problematic but it is an upper limit. We even know from the merger scenario that MACSJ0717 is not in the situation described by M3b. It is therefore consistent with the other models (it should be more clear now in the table).

- why presenting maps for model M1

Because it is the most simple model (and we can only show one case in a letter). In addition, n\_e and P\_e maps are proportional to observables in the case of M1.

- ".. relativistic corrections has a negligible impact on our results." - This comments seems to me a bit strong, can you quantify it please?

The relativistic corrections are not negligible obviously. However, using XMM, CXO, or MGW temperature maps to compute relativistic corrections have a very small impact on the results. This was checked in the kSZ paper for CXO vs XMM. Even if the difference in the temperature used to compute relativistic correction is ~20%, the change on the results will be ~1.5% peak to peak (locally on the map), so even much less for the cross-correlation.

4) Results

- Morphology: It would be nice to be able to quantify  or show a bit more the consistency between the maps. For example the contours of the SZ temperature map could be overplotted on the X-ray maps.

??? It is the same color bar, same contours, same size, same mask … Adding T\_gmw contours on T\_x map does not help, but only add confusion.

- P\_{0} -> \hat{P}\_{0}

ok

- Table 1; can you add uncertainties on the measure slopes?.

Done in the caption, it’s 2% for everybody and the table needs to remain small.

- Fig3. are you using a robust fit estimation or a simple linear fit?. For the Chandra data the fit does not seem to follow the data, in particular for the with kSZ case. It also looks a bit non-linear.

We use both simple linear regression (macsj0717\_thermo\_simple\_regression.pro) and MCMC fit (macsj0717\_thermo\_mcmc\_regression.pro). Both give exactly the same results. We account only for error bars on the y axis.

I checked and the plots seem fine. Your eyes are probably driven by the red dots at high temperatures, which are not hidden by the green dots.

We could assume higher order polynomial relation, but this would be another story. But be careful of modeling effects here. For example, the kSZ correction in region C affects only the high T part of the plot. Such effect could look like non-linear features. My feeling is that the best-fit kSZ model is good in region B but too strong in region C. If true, the most accurate plot would be green dots at high T and red dots at low T…

 5) Discussion and conclusions

- "Tgmw and TXMM are in excellent agreement .." --> this seems to be too strong, can you quantify in terms of chi2 or other goodness-of-fit statistics

Ok, « good » is enough for me, but you need to put this in the context: this is the first T map derived from SZ data, and despite all the complexity, we find a cross calibration close to 1 with XMM spectro. I think this is not too bad at all so we should be happy.

- "The tSZ+X-ray  imaging results are stable within less than 10 % .." This sentence needs to be reformulated. Do you mean that the correlation between the SZ and X-ray temperature maps is consistent within 10 % including uncertainties and systematics in the modelling?. If so the results for model M3b are not consistent with this statement.

Done. M3b is an upper limit, so it is consistent.

- "For a perfectly spherical cluster the ratio TX/Tmgw would give access to absolute calibration of X-ray temperature." -> I disagree on this sentence because of my comment on equation 1. Have you proved this using simulations ?

I do not agree with your disagreement. We are talking about weighted quantities here. (See response on Eq 1 comment.)

- "The noise in Tmgw map is comparable to that obtained from XMM-Newtown and Chandra". -> I do not understand this as you said before that you can not estimate uncertainties for the X-ray temperature maps.

The scatter between CXO and XMM is larger (but comparable) to the errors on Tgmw. Therefore, we know that the noise levels are comparable, even tough it is qualitative. Now we have an estimate of the Tx error, so it is fine.