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

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**Marco De Petris – August 11th, 2016 – Reply August 26th, 2016**

3.2. X-ray electron density mapping

The internal structure of MACS J0717.5+3745 is increasingly refined from model M1 to M3, but we find good consistency between all three models.

Why at the end all the comparisons in the paper are done with M1? See also General Comments 1).

Since this paper is a letter, it is not possible to show the full comparison and we have to select a reference.

M1 is a good reference because it is the simplest model. In addition, the advantage of M1 is that the maps of

n\_e and P\_e are directly related to the observables, so they are better for visualisations purpose (not true for

the temperature map, however).

I agree. That’s one reason that could be explicitly written to motivate the choice of using M1 as a reference.

3.4. Gas-mass-weighted temperature mapping

In the second member of eq (6) the factor sT/(mec2) is missing.

You have already introduced, and defined, the gas-mass-weighted temperature just after eq (1).

I don’t think there is a mistake here. This is just the perfect gas law integrated along l.o.s., indeed similar to

eq 1 (+eq 4 and eq 5).

About eq (6), you are right! …I see *y* (instead of P) even where there is not….sorry!

Regarding the text “Tgmw is gas-mass-weighted.”, just after eq (6), it is a repetition because you have already defined it after eq (1) but probably you believe that *repetita juvant* …

4.2. Temperature comparison

In eq (7) you are considering kBTgmw and kBTXMM/CXO and not Tgmw and TXMM/CXO; in fact a and s are reported in keV.

Ok it is better with K\_B, but this doesn’t matter (only beta and sigma changes unit then). The coefficient **a** is

unitless.

Several times T and kBT are used in the same way but in eq (7), exactly due to the fact that sigma is in keV, it’s better to explicit kB, as you did now. I agree that a is dimensionless, being the slope.

Has β always the same value for CXO and XMM in eq. (7)?

Yes within a few per cent, but the changes in beta are dominated by systematics in the model (similar to

alpha)

This means that you have almost the same zero offset (P0) for the Tgmw map for NIKA-CXO and NIKA-XMM but different for each model.

Chandra temperatures are greater than XMM ones and an offset (in Te) could probably change β values and, consequently, the a values.

alpha and beta are degenerate, but in any case we are dominated by systematics in the model. This was

checked by changing the fitting method to produce full alpha-beta 2d likelihoods via MCMC fit. Then, we

assume a linear relation between the temperature measurements, so what you say is included in our

procedure. Obviously if the assumption is wrong this can affect the parameter values.

What I was referring is the possibility that in the linear relation between the spectroscopic temperatures of CXO and XMM could be included an offset, *i.e.* a normalization factor in the linear fit (probably difficult to explain). At the moment you are assuming that TXMM = aCXO-XMM x TCXO. I don’t know if the fit (Fig.3 on the left) is better including a normalization factor.

amgw-X 🡪 agmw-X

Ok

Table 1.

The value of the slope, aCXO-XMM, is referring to the XMM-CXO one.

See also Figure 3 Left: Chandra versus XMM-Newton spectroscopic temperatures. I would expect in the legend in that plot TCXO = 1.172 TXMM, otherwise you have to reverse the axes.

Indeed, the axes labels are inverted.

5. Discussion and conclusions

Tmgw 🡪 Tgmw

Ok

**General comments:**

* “The tSZ+X-ray imaging results are stable to within less than 10%, depending on the choice of the eff  model”

Due to the fact that the results are stable, why do you prefer to apply M1 throughout the paper? In that case you are applying the eff value as derived from other SZ observations (Bolocam and SZA). In order to be independent from previous observations, with the idea to possibly apply this approach to other high-z clusters, it should be more valuable to employ NIKA (+X-ray) data alone.

Well 10% is stable but not super stable; we are still limited by modelling systematics. We can get a

l\_eff value assuming M1 with NIKA alone, but it was already done in previous work so we can also

use it.

Did you estimated l\_eff from NIKA data alone? and if yes, it is consistent with Mroczkowski et al. (2012) value?

Moreover, as specified, M3 is clearly the only approach that deals with the substructures present in the cluster.

True, but M3 involves other assumptions and I would not bet on my life that M3 is better than M1 and

M2. This cluster is very complex and the only thing we can do with confidence is to check variations

of the results under variations of the assumptions.

I agree that M1 is the simplest model and, for sure, the most appropriate model to be applied to spherical clusters. In the case of a strong morphological disturbed cluster, such as MACSJ0717, I think that it should be interesting to show the Tgmw as derived also with M3 more accurate to map substructures and clumpiness but, I agree, probably in a Letter is too much.

M1 and M2 are both assuming spherical distributions of gas density and pressure. I think that this is useful if you would like to infer a cluster-center radial distribution of the gas temperature, but this does not seem your goal.

Yes, but they allow us to test the stability of the results. No model will ever describe MACSJ0717

perfectly.

See before

* The assumption of “*slab approximation*” (i.e. Te constant along l.o.s.), as described in Appendix of Mroczkowski et al. 2012, is based on cluster-center radial profiles of Compton-*y* and SX with appropriate powers.

In the case of this unrelaxed cluster, a 2D offset is present between the peaks of Compton-*y* and SX. Could be this an issue to assume the *slab approximation*?

What we measure is the temperature weighted in some ways along the line of sight. We do not

assume that it is constant except when computing SZ relativistic correction but this has negligible

impact on our results.

* What about the Compton-*y* over sqrt(SX) map? i.e. the sqrt(eff) map, under the assumption of a constant Te along l.o.s. …

I probably missed something, but I could not find mention in the text of comparisons between eff maps, assuming the 3 models, and that map.

Indeed, we could assume that T\_x is perfect and compute a map of l\_eff from S\_x + SZ, but this is

not the approach we chose here.

Yes, probably that is the fourth approach, *i.e.* M4, to generate a map of l\_eff as the ratio of y2/Sx starting from the maps of kBTXMM/CXO. Are you planning to extend the analysis in this regards in the future? The comparison of l\_eff maps derived from the different models is interesting to highlight the discrepancies (if relevant) among them depending on the morphology of the cluster.

* Just a detail …, personally I prefer the Sayers’ definition of , as the *effective line-of-sight extent of the ICM* instead of the original by Mroczkowski as the *effective electron depth.*

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Unfortunately I have not been convincing in this regard… ;)