Mapping the kinetic Sunyaev-Zel'dovich effect with kinetic inductance detectors in MACSJ0717.5+3745

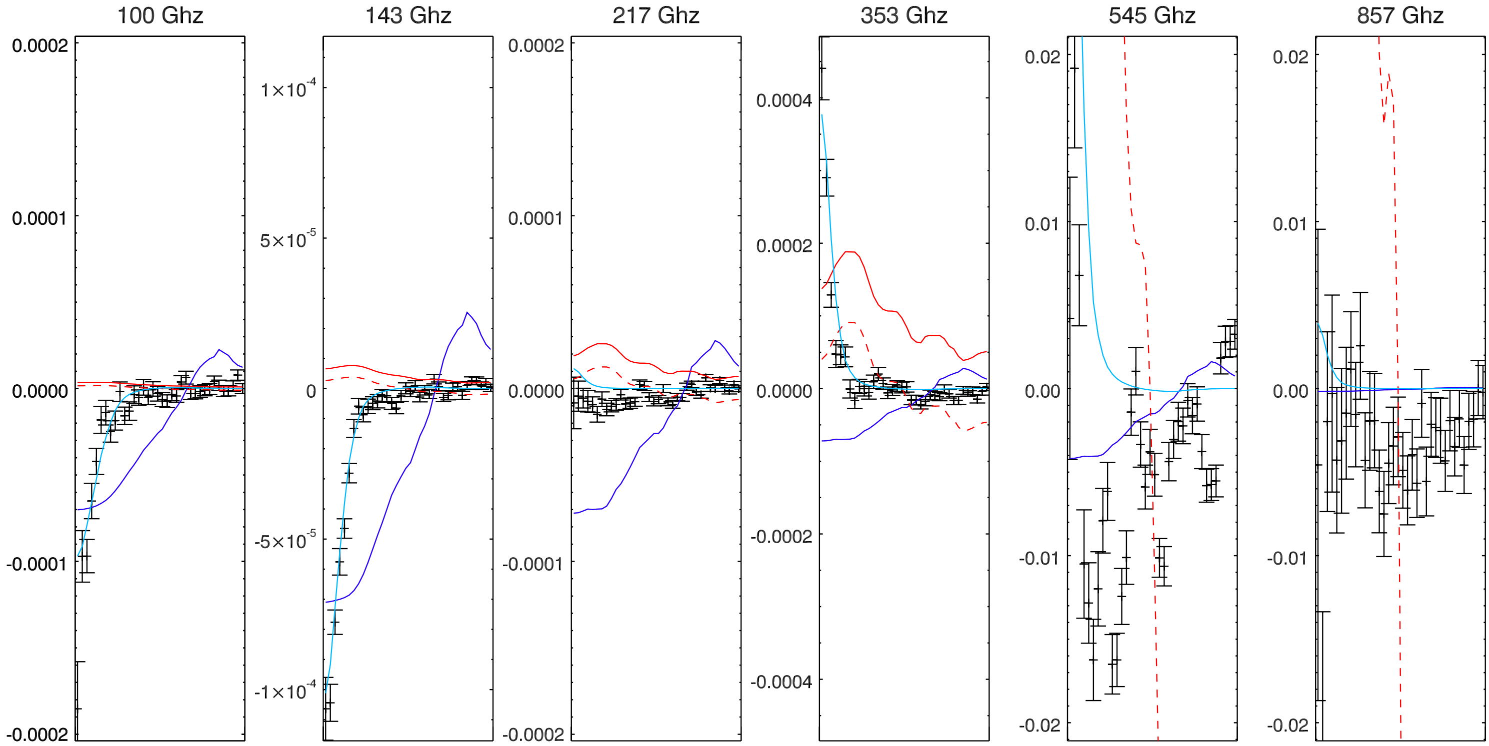
Comment (and response) from Hervé Bourdin - 04/05/2016

Iacopo provided me with the last version of your paper and asked me for comments about the X-ray analysis. Well, both the SZ and X-ray the data analysis parts of this work look quite nice, so I do not have any important comment about them. I should confess that I feel less confortable with the X-ray+SZ combination and the final interpretation of the results. It may be partly my fault if we did not talk about this earlier but let me provide you with few of my thoughts in any case.. hoping this to be helpful.

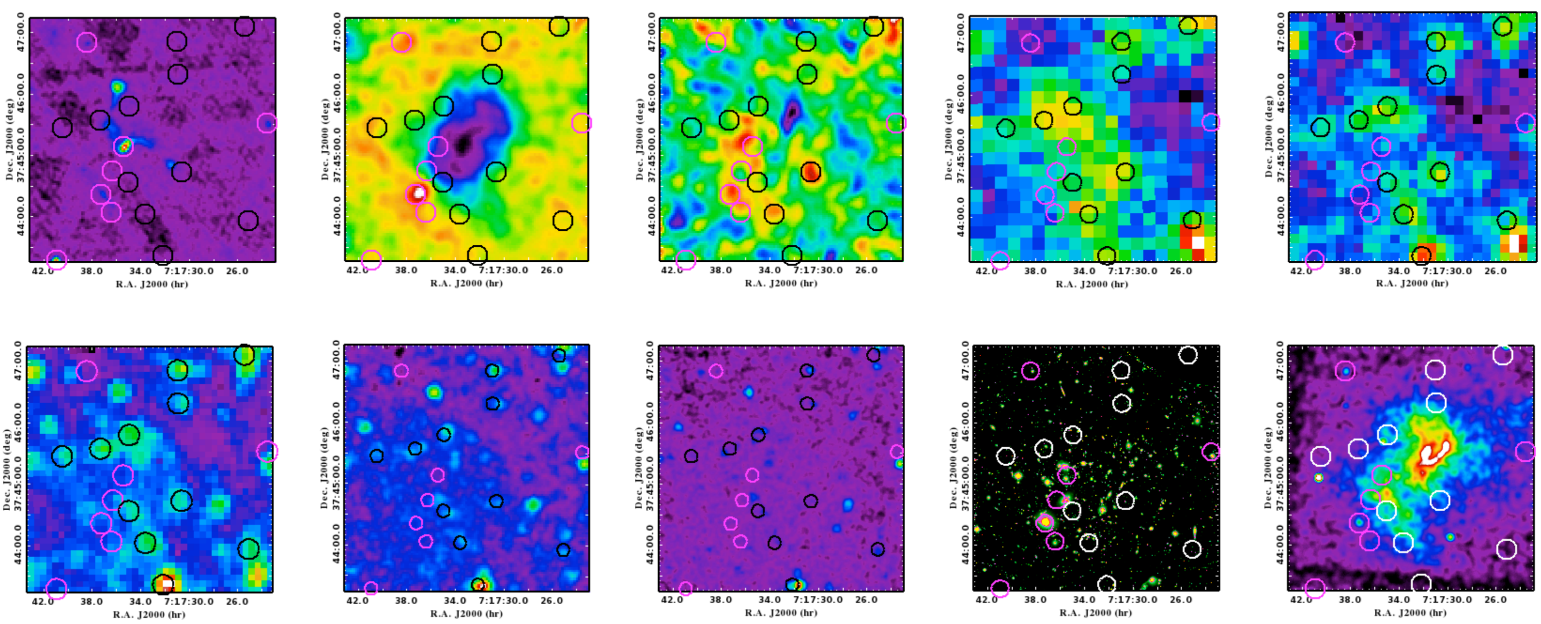
Thank you very much for your comments. They are very interesting. You will see my detailed answer below (in blue) but concerning the modeling in general, I want to stress a few things. Clearly the main result of this work is to obtain a kSZ map, which is almost model independant. Because of the complexity of the cluster, any modeling will be limited and the solution adopted here is relatively simple but it is already quite complicated to fit it to our data. As you will see, I tried several options and this model is to my opinion, the least bad that I could come up with. Finally, this kSZ mapping is the first one and this is very nice, but our signal to noise remains limited. Therefore, I am really in favor of keeping things simple if possible because I don't want to risk to over interpret what is observed.

1) SZ data analysis; The detection of spatially resolved kSZ fluctuations is the most important result of the work, and it looks convincing! However, one usually wonder in such cases to what extent astrophysical contaminants might be responsible for the observed spectral distorsion evidenced in Fig. 4 (in particular because NIKA has only two bands..). My understanding is that contamination from radio and infrared point sources have already been investigated, together with extended contributions from the CMB and CIB. A possible contaminant that may remain is the extended thermal dust emission from the cluster itself. Similar to what is observed in many distant clusters of the Planck catalogue (e.g. Planck intermediate results. XLIII), the Planck 353 Ghz frequency map show an infrared excess with amplitude of about half of the amplitude of thermal SZ Compton parameter toward the cluster peak (see attached plot). Of course the angular resolution of NIKA is not the same, and I guess most of this contribution is resolved in point sources as discussed in section 3.3. Maybe simply saying that Herschel maps do not show any extended infrared/submm emission that would be spatially correlated with the strongest kSZ fluctuations (or showing them similar to what have been done in Adam et al, 16) would help and convince the reader that these fluctuations are indeed real.

Bellow is the residual observable on the Planck HFI frequancy maps toward MACSJ0717 after subtraction of the CMB (dark blue) and infrared (red) fluctuataions at angular scales lower than 10 degrees. x axis is in unit of r500. It is well fitted by a thermal SZ model (light blue). Note the infrared excess clearly visible at 353 Ghz.



Indeed, the astrophysical signal is one of the main systematics in such case. We have accounted for: galactic foreground (Sync., f.f., dust, which are negligible extrapolating Planck), CMB (negligible at the our scales), radio diffuse cluster emission (negligible extrapolating litterature results), radio point sources and infrared point sources. The point sources are well identified from Hershel and lower frequency data. Their removal is not perfect, but thanks to the resolved nature of the NIKA observations, we can check that they are not coincident with the kSZ.
The IR excess seen in Planck is probably due to these same sources, but as you say we can also imagine a contribution from dust wihtin the cluster ICM, which would be more diffuse. Even though I am pretty sure that even if it is a significant signal, it would not be important because it would be a diffuse signal at ~arcmin scales, I don't know how we could check this with existing data.
So to convince the reader concerning, we could show the following figure (I think this is what you mean: FIRST 1.4 GHz, NIKA 150 GHz, NIKA 260 GHz, SPIRE 500 um, SPIRE 350 um, SPIRE 250 um, PACS 160 um, PACS 100 um, HST, Chandra), but the confusion is quite strong at 500 / 350 um (so difficult to differenciate point sources from diffuse emission) and 250 um is already pretty far in frequency, so I am not sure it is much more convincing.
In any case, I will add something in the text about this.



2) Joint SZ+X-ray analysis, gas density/velocity modelling; The main concern I could find here is that the 3d modelling of the gas distribution proposed in section 6.1 is extremely idealised, and does not seem to be strongly constrained by the X-ray/SZ data.

I fully agree with this statement and this is clearly limiting our constraint on the velocity. However, modeling such cluster will never be perfect. I tried 3 other approaches and the one chosen in the end is the less bad to my opinion. The two others are:

1. Build a tSZ template based on Sx and Tx:

$$I_\nu^{\rm model} = f_\nu \tilde{y}_{\rm tSZ} + g_\nu \sigma_T \sum_i \left(\frac{-v_z^{(i)}}{c} \int n_e^{(i)} dl \right)$$

with

$$\tilde{y}_{\rm tSZ} = \sigma_T \frac{k_B T_x}{m_e c^2} \sqrt{\frac{4\pi(1+z)^4 S_x}{\ell_{\rm eff} \Lambda(T_x, Z)}}$$

$$\ell_{\rm eff} = \frac{\left(\int n_e dl\right)^2}{\int n_e^2 dl}$$

This is the approach followed by Sayers et al. 2013. The main problem is that l_eff is not known. Sayers et al. assume that it is a scalar and let the amplitude of the tSZ template free, therefore marginalizing over l_eff. This might be OK for Bolocam at ~45" FWHM, but using toy models, I found that we can have spatial variation of l_eff of more than 50% at our scales, only because the l.o.s coordinates of the clumps are unknown. This generates cross terms in n_e^2 which depend on the relative distance between clumps along the line-of-sight. In addition, the kSZ component still has to be described by some model of the gas density (beta model for Sayers et al.), and therefore the model is not internaly consistent between tSZ and kSZ contributions

2. Fit the same model as in the paper, but using Sx and y_ksz for the fit.

$$y_{\rm kSZ}^{\rm model} = \sigma_{\rm T} \sum_i \left(\frac{-v_z^{(i)}}{c} \int n_e^{(i)} dl \right),$$

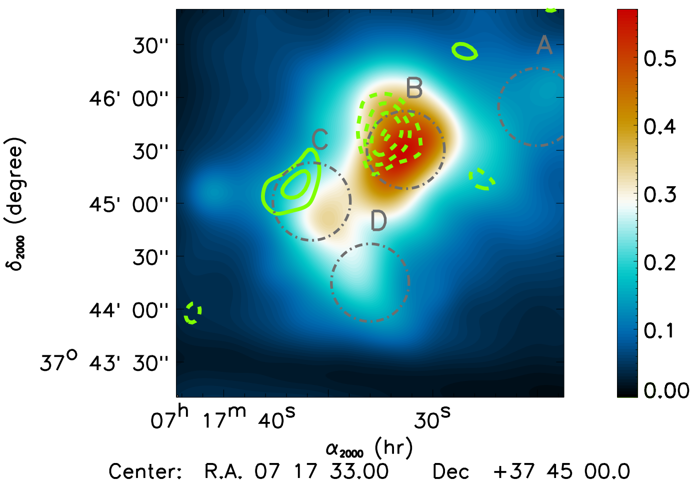
$$S_X^{\rm model} = \frac{1}{4\pi (1+z)^4} \Lambda(T_e, Z) \int \left(\sum_i n_e^{(i)} \right)^2 dl,$$

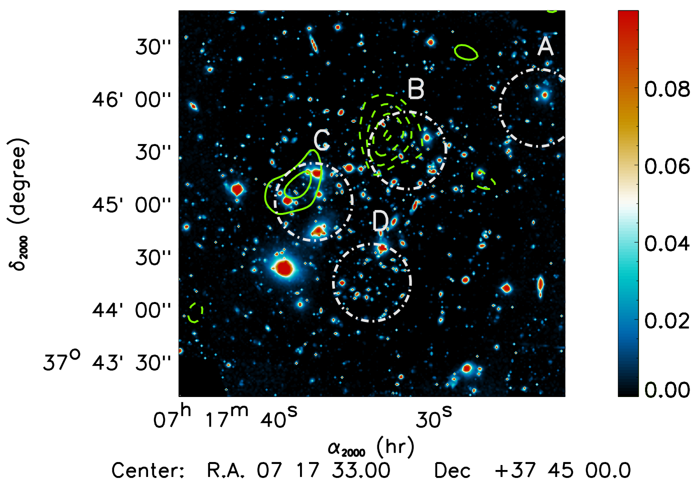
The main problem is that the l.o.s coordinates of the clumps are not known, and depending of their values, the cross terms between pairs of clump arrising from squaring the density, changes significantly (same as previous case for l_eff). In addition, I don't like fitting simultaneously two maps with very different statistics (+systematics which in don't know for Sx) and very different signal to noise.

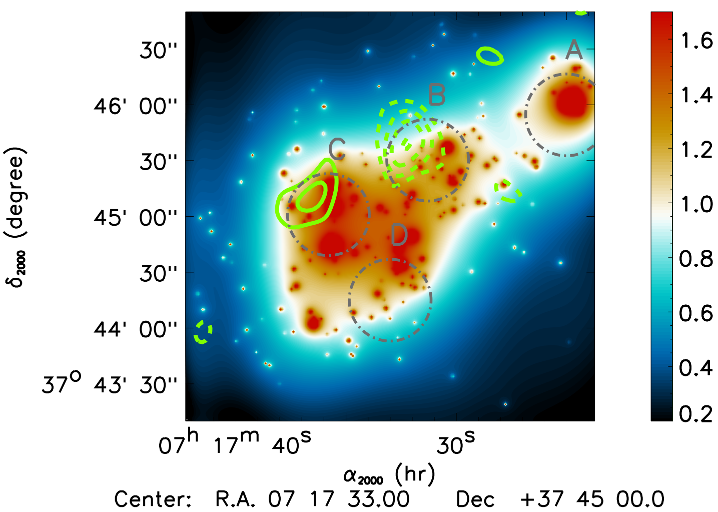
Briefly:

-- By looking at the X-ray surface brightness images, I am not fully convinced that the decomposition in four subclumps (A,B,C,D) is the best/unique solution that would fit the X-ray+SZ data. I understand that this decomposition originately comes from lensing works, but of course in a major merger the gas distribution (whether 3d or 2d) is not supposed to match the dark matter. If indeed the X-ray/SZ data hints at such a decomposition, this should be clarified in the text. In particular, the position of the four subsclusters is claimed in section 2.3 to have been adjusted to in order to match the peak of the Chandra wavelet map, but this is not evident on Fig. 3.

Clearly the decomposition in four subclumps is not unique and maybe not the best, but it is difficult to find satisfying alternatives. And in fact (and surprisingly for such complex system) it is not so bad since our data are quasi consistent with this model. Even the fit of S_x with four beta model gives residual was giving ~15% only (model 2 discussed above). I should stress that the position of the regions defined from lensing/X-ray are adjusted by hand in an arbitrary way. Basically, I took the lensing coordinates and moved region D more to the south because their is a clear offset between lensing and X-ray for this clump. However, they are only used in plots as reference coordinates for illustration (and for the spectra in figure 4). In the model fitting, the cluster centers are allowed to move. Maybe you do not have the figure below in your version of the paper (figure 6 now, showing the regions versus the Chandra imaging, HST and lensing):

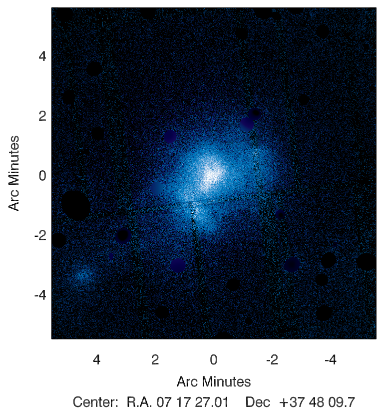


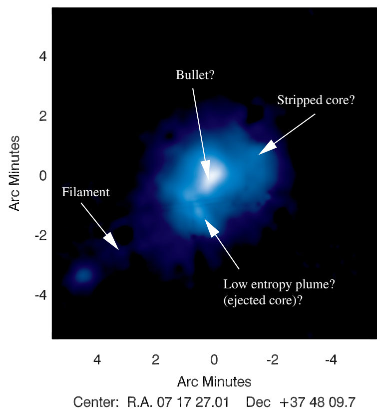


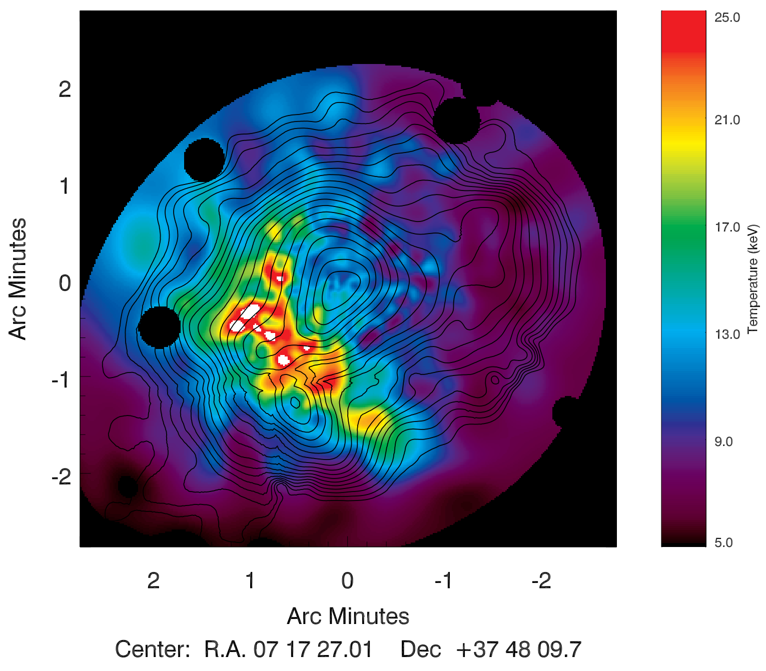


-- The X-ray images also show a number of anisotropic features (stripping tails, filaments, see attached image) that suggest that the superposition of four spherical beta-models is a strong idealisation. In particular subclusters A and D seem to be followed by low entropy tails, which suggest they may be dense stripped/truncated cores.
I agree, but modeling this kind of things is far beyond anything we can do. Moreover, we will not have the sensitivity to constrain it from SZ data.

-- The projected temperature map can not be fit by the superposition of four isothermal subclusters. Instead, it seems to reveal a high temperature/entropy bar roughly orthogonal to the major merger axis, separating subcluster B from C+D. This looks similar to shock-heated or at least adiabatically compressed regions observed in bimodal pre-mergers like A521, A3921, A1750..
Again I perfectly agree. However it is not very very bad (just a bit bad) to say that the temperature is more or less constant within the 4 regions independently. At first I was using the Tx map in equation 9 of the paper instead of isothermal clumps, but then the model is not self consistent (between pressure and temperature).
Ideally one could perform a joint fit of the 3d gas density to the NIKA frequency maps, X-ray projected surface brightness and temperature maps, but this is probably too much work at this stage.
The model you propose is very similar to model 1 descibed above, but see the reason why I don’t want to do this above.
Alternatively, I am wondering if idealising the 3d temperature distribution of the gas as the sum of identical 2d slices that would match the X-ray spectroscopic temperature map could not be quickly tested. This is another strong idealisation but it could help us explore the systematics of the final velocity values reported Tab. 7 and velocity/optical depth maps of fig. 9. I am wondering in particular to what extent any temperature gradient in the East side of the hot bar of fig. 3 contributes to the gradient visible in the eastern side of the velocity map of fig. 9. As I will detail below, I am also wondering if sucluster B is indeed unique or rather the projection of two subclusters along the line of sight.
I am not sure to understand, but I should stress that despite being to simplistic, our model is already made of 26 parameters and it is not so easy to fit it. Therefore, I am not sure that adding any extra complexity would result in better results in the end.



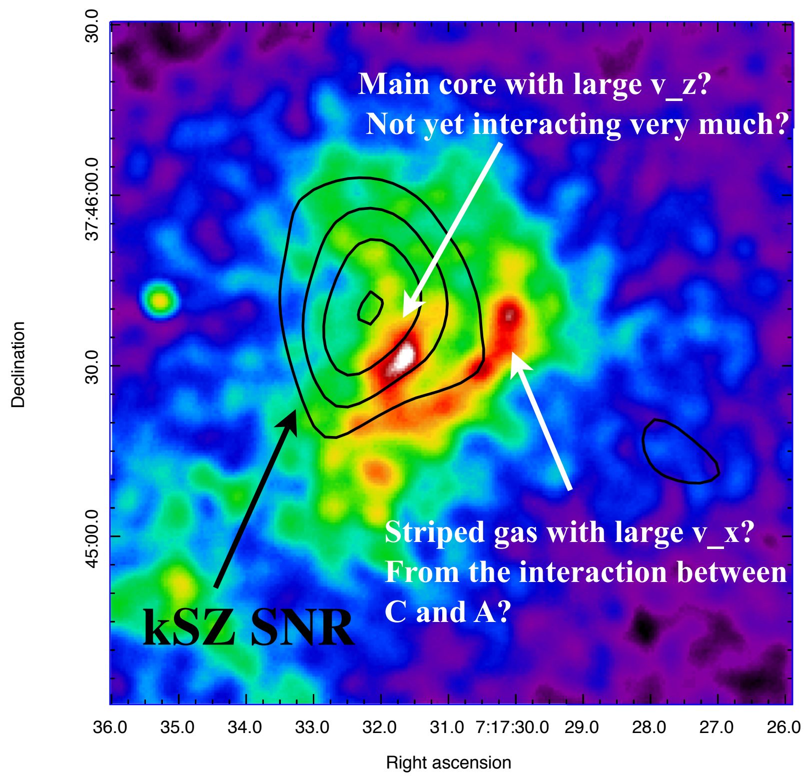




3) Discussion. The evidence for a high velocity motion of sucluster B along the line of sight is to my mind the most significant result of this work. However, following the proposed four components modelling subcluster B also coincides with the dominant/central gas component of the interacting system, so a naive question that comes out is toward what subcluster B is falling/moving at this velocity. One could also note that this high velocity motion along the line of sight is quite unexpected given the presence of a S-E / N-W filament at large scale, and to some extent given the evidence for a high entropy/temperature bar separating B from C+D. Given the fact that the X-ray image does not show a unique peak toward subcluster B, I am wondering if an alternative model including two components coinciding with B along the line of sight (one that would move toward the observer, another that would move along the merger axis in the plane of the sky) could not be investigated.

Concerning your thought about the unexpeced velocity for B. I understand your statement but the filament could very well have some orientation along the line of sight, which we may never know because of projection effects. Sub-cluster B could also have some high velocity component perpendicular to the line of sight which we don't measure. In addition, B is quite compact and therefore shows up strongly in X-ray but the main cluster is C in terms of mass at least. So we can discuss this a little bit but I am not so much surprized about this high velocity.

Concerning the sub-structure within group B, indeed I suspect that there are multiple components (at least two: east and west peaks). The kSZ signal is better aligned with the east component, which reinforce what you say (see image bellow). If we include two components, the fit will be sligthly better, the velocity higher, but the two component parameters will be completely degenerated. The fit is already difficult because of degeneracies, so adding one component near B seems not feasible to me. However, it can be very interesting to discuss in details and maybe even showing an image as this one.



Another thing which could also suggest the multi component idea is the following. ACCEPT provides a density deprojected profile (almost centered on B, and including the contribution of all sub-sub-cluster in B) which I fitted with a beta model and I used the constraint on ne0 as a prior for our fit. The tau-v_z degeneracy is broken and the new likelihood agrees with the previous one within less than 2 sigma (see the plot bellow). With the prior, however, the optical depth tends to be higher than what the previous fit prefers, and this could be due to this secondary component which l.o.s velocity is small but included for wrong reasons in ne0. Our SZ data on the other hand would be less affected by this secondary clump because the SZ is just given by the sum of the clumps will the X-ray emission is gibrn by the sum of clump squarred.