Answer to the referee report

<u>**Referee Report**</u>

I'd like to congratulate the authors on their new measurements and recently commissioned technology used to carry out the research. This work certainly merits publication. However, there are several issues, some minor and some more serious, which need to be resolved before publication. I have itemized them throughout the text in the sections to follow.

A general comment:
- I understand that many of the authors do not speak English as their first language, but in several sections the use of language is so poor it leads me to speculate that they were not even proofread before submission. This can take away from the scientific interpretation. Before re-submitting this manuscript it needs to be gone over in detail to edit the use of language and clarify the sentences which I have pointed out in the below section. This really should have been done long before it ever got to the referee.

<u>**Answer**</u>
First of all we would like to thank you for your very deep reading that truly helped us improving our analysis and the content of the paper.

You will find below the answer to your comments. Our response is in bold face, such as the modifications in the text of the paper. We also provide few new figures that we prefer not to include in the paper but that are useful for this answer.

Best regards.
Rémi Adam, Barbara Comis and Juan-Francisco Macías-Pérez on behalf of the authors of the paper.

Itemized comments.

Abstract

Context: "Clusters of galaxies provide precious informations on the evolution of the Universe and large scale structures"
"precious informations" is qualitative, vague and grammatically incorrect, please reword this opening.
***** **"precious informations" has been changed to “valuable information”**

Conclusions:
"will be a well-suited instrument for in-depth studies of the Intra Cluster Medium from intermediate to distant clusters and so for the follow-up of recently detected clusters by the Planck satellite"
This sentence is confusing and poorly worded.
suggestion: "will be well-suited for in-depth studies of the Intra Cluster Medium in intermediate to high redshift clusters and the follow-up of clusters newly discovered by the Planck satellite"
***** **suggestion applied**

Introduction

The sentence "They are classically probed .... observations" needs improvement. The list of citations in this context should either be expanded or removed entirely. Each observable has a single citation to a

review article, at the very least the chosen citations should contain an "e.g.", or at the end of the sentence, include a clause as you have done later for the tSZ effect, "for reviews on cluster observables, see....." Each observable should be tied to the physical components of clusters which you are describing. Bremstrahlung from the ICM is traced by X-ray, Optical and IR traces the stellar emission in the member galaxies and, lensing surface mass density maps are produced from multi band optical-ir data.
***** **corrected**

"complementary and powerful"
- remove "and powerful". These extraneous qualitative descriptors are found throughout the text and are not scientific.
***** **the adjective powerful has been removed**

"ICM properties of X-ray (sensitive to the density squared)"
-This sentence is also imprecise. You specify that the SZ depends on the line of sight integral, but do not state that for the X-ray, which of course is a line of sight integral as well. The X-ray emission is also dependent on temperature, albeit much more weakly than electron density. The manner in which it is worded here makes it sound like X-ray traces only the square of the density.
***** **The sentence is now "… ICM properties of X-ray (sensitive to the line of sight integral of the density squared and the square root of the temperature) …"**

"tSZ flux does only depend on the angular size of the observed cluster and not on the distance to the source"
-This is another poorly worded sentence. The point you are trying to make here is that SZ does not suffer from cosmological dimming effects. Saying that SZ flux depends on angular scale is misleading. The SZ flux from a given cluster will not change if you move it from low redshift to high redshift (i e change the angular size), it will just change the flux per pixel.
***** **The sentence has been changed to "In addition, unlike other observational approaches, the tSZ signal is not a affected by cosmological dimming. Only the angular size of the observed cluster depends on the distance to the source."**

"It consists of two arrays of 132 and 224 detectors, observing at 140 and 240 GHz, with resolutions of 12.5 and 18.5 arcsec." You need to make this sentence coherent with respect to the order you are listing. This sentence reads as if the 140GHz band has better resolution, which does not make sense.
***** **12.5 and 18.5 arcsec have been inverted**

Previous observations of RX J1347.5-1145

"RX J1347.5-1145 is one of the very few clusters that has been intensively observed in many wavelengths, including sub-arcmin resolution."
-This needs to be clarified. There are many clusters which have been "intensively" studied across the spectrum from X-ray to IR. RXJ1347 is the most widely studied cluster with subarcminute SZ.
***** **Sentence has been replaced by "RX J1347.5-1145 is among the clusters that have been intensively observed at several wavelengths, and the most widely studied using tSZ at sub-arcmin resolution."**

"It is the most luminous X-ray cluster ever observed"
-This superlative sentence clearly warrants citation.
***** **The sentence is now: It is the most luminous X-ray cluster of galaxies known (e.g. Allen et al. (2002)).**

"From X-ray observations, this cluster was thought to be a dynamically old relaxed cool-core cluster with an extremely strong cooling flow, due to its very spherical morphology and peaked X-ray profile (ROSAT; Schindler et al. 1995, 1997)."
- This is not quite fair to X-ray. You need to specify that this conclusion was inferred from Rosat measurements in the text. As soon as Chandra observations became available the SE enhancement was clearly detected in X-ray.
***** **We corrected the main text accordingly**

"shift between the X-ray and tSZ signal maxima (Pointecouteau et al. 1999; Komatsu et al. 2001; Kitayama et al. 2004)."
- The location of the absolute maximum in SZ is still debated because of the effects of the point source removal. What is clear is that there is a secondary maximum in SZ to the South east which indicates injected thermal energy from the merger event. This point comes up throughout the paper and in many cases the literature is improperly cited. Plagge et al, who exploit the point source's characteristics in the UV plane to subtract it, claim that the absolute pressure peak is coincident with the SZ. Mason et al, Korngut et al, Komatsu et al and Kitayama et al make no quantitative measurements towards the AGN as its contamination is not well separated in single dish measurements.
***** **Corrected in the text.**

"As a matter of fact, it is a perfect illustration of the complementarity of tSZ and X-ray (and other wavelengths) observations."
- Needs to be reworded, remove the word "perfect" and the expression "matter of fact".
***** **Corrected**

Observations with NIKA

"with maximum transmission at about 140 and 240 GHz"
-Give the measured peak frequencies and their effective bandwidths.
***** **We give the exact maximum transmission and provide more precise bandwidth.**

"effective fields of view of 1.8 and 1.0 arcmin"
-FOV needs to be specified in solid angle. From figure 1 it looks like what you are quoting here is the length of a side of the FOV. Change the text to say either "1.8x1.8 arcminutes" or "3.24 arcmin^2".
***** **Corrected**

All quoted sensitivities have units written as mJy.s^{1/2}. Please correct this latex error.
***** **The sensitivities are now written as mJy s^{1/2}. We think you wanted the dot to be removed but we are not sure.**
Also you quote the sensitivity "per beam" but do not mention what the spacing of the detectors is. In the last paragraph of this section you mention that the pixels are .77F\\lambda and .8F\\lambda apart. This should be mentioned here, and discuss how this factors in.
***** **The spacing between detectors has been moved here.**
What is an estimate of the photon noise? Are you dominated by detector noise? It seems like amplifier noise is also a significant contributor based on your earlier rationalizations of the low detector yield.
***** **We are not photon noise limited; the expected photon noise for background conditions during the observations is 5 mJy sqrt(s) and 7 mJy sqrt(s) for the 140 GHz and 240 GHz channels respectively. For the 240 GHz channel, we were limited by the cold amplifier noise that did not work correctly for this run. For the 140 GHz channel, in particular for the tSZ**

analysis, we are limited by residual correlated noise both from atmospheric emission and electronics. This has been clarified in the text.

"This rather sensitivity worse than expected and the small number of available detectors of the second band, in the case of this campaign, is due to the dysfunction of a cold amplifier."
-Come on.......... The grammar used in this sentence is so poor that it is difficult to understand. This needs severe rewording and clarification. It is slightly offensive that this sentence was submitted for publication.
***** **Corrected in the text**

"These quantities can then be used to reconstruct the shift of the resonance fre- quency which is to first order proportional to the absorbed optical power with accurate photometry (Calvo et al. 2012)." The inclusion of "with accurate photometry" in this sentence does not make sense to me. What I think you mean is that "I" and "Q" can be used to measure incident optical power which can then be used for accurate photometry on the sky. Clarify the intention of this sentence.
***** **The sentence is now:**
"These quantities can then be used to reconstruct the shift of the resonance frequency. The latter being proportional to the optical power absorbed by the detectors, it is used as a probe for accurate photometry (Calvo et al. 2012)."

"a few hundred μm thickness high resistivity silicon substrate."
- Give the measured thickness instead of "a few". What is the resistivity of the substrate?
***** **We give the following values:**
180 µm and 275 µm thickness silicon substrate at 240 and 140 GHz respectively, with high resistivity: >5000 Ohm cm for both wavelengths.

"only the left part of the field was covered due to software mistake"
-Give a direction "east" or "west" instead of "left".
-Change "due to software mistake" to "due to an error in the control software". This type of grammar mistake should have been corrected long before submitting to a journal for publication.
***** **Corrected**

"360 × 180 arcsec^2 (azimuth × elevation)"
-Once again the region of sky is given in incorrect units. It is either 360 x 180 arcsec or 64800 arcsec^2. The same is true for the elevation scans.
***** **Corrected**

Pointing, calibration, bandpasses and beam

"The flux of the planet was inferred from a frequency dependent model of the planet brightness temperature taken from Planck Collaboration et al. (2013c). Typically we obtain brightness temperatures of 113 K at 140 GHz and 94 K at 240 GHz for Uranus1"
-This is cited in a confusing manner. The Planck paper provides measurements of the brightness temperature but cites the model from Moreno 2010.
"We obtain brightness temperatures....", sounds like you are measuring the brightness temperature with NIKA, but if you are using Uranus as a primary flux calibrator the brightness must be assumed by the model. Clarify your primary calibration process.
***** **Corrected accordingly**
"The final absolute calibration factor is obtained by fitting a Gaussian of fixed angular size on the reconstructed maps of Uranus (representing the main beam)."

-Presumably you are fitting the Amplitude of the Gaussian. This should be stated. In the previous sentence you say that the angular size of the planet is accounted for but then state that you fix the size of the Gaussian to be the main beam. The planet has an intrinsic diameter of 3-4 arcseconds which is not completely negligible. How much power is in the sidelobes?
***** **Corrected. The beam dilution effect even for a 4 arcsec diameter planet is negligible with respect to the main beam size. We have quantified this effect as shown in Figure 1 below.**

The fraction of power in the sidelobes was given two paragraphs later and in table 2 quoted as secondary beam fraction. We have reorganized the paper to improve the readability.

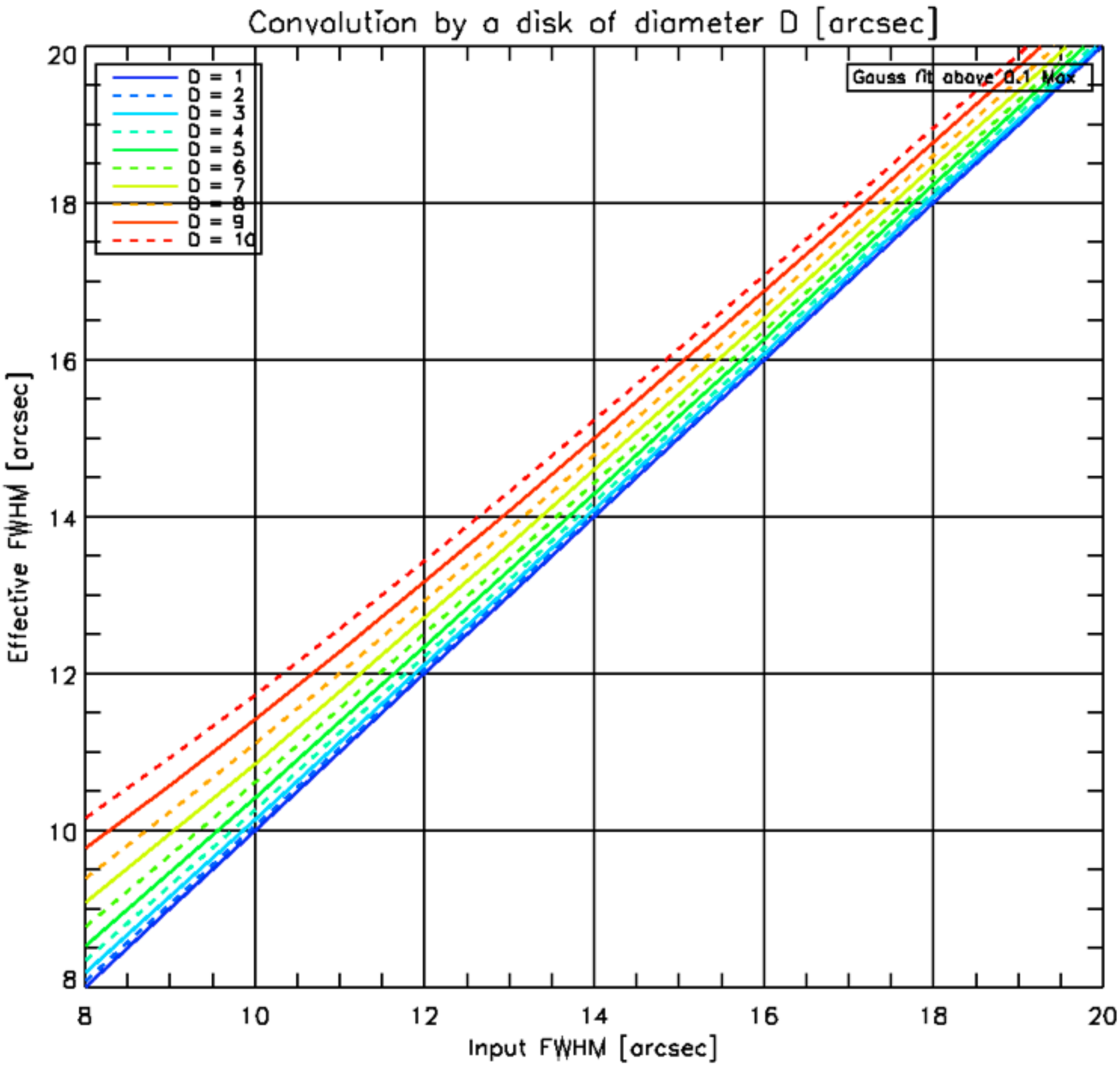

Figure 1: Effective FWHM of the planet convolved beam as a function of the main beam.

-"Using the IRAM 30-meter beam pattern from Greve et al. (1998)2 we extrapolate the angular profile of the beam from 100 arcsec to 180 arcsec" Is the beam pattern measured 15 years ago with a totally different receiver/primary illumination really accurate enough for this purpose? A figure which shows a NIKA measurement of Uranus, the Gaussian fit, the Greve et al template and your extrapolation is clearly necessary to justify your assumptions and illustrate your calibration process. ***** **We have used the beam pattern of the 30m telescope as measured by Kramer et al.**

(2013) (K13) on the Lunar edge using the EMIR receiver.A reference to this paper is now given in the main text.

We present in Figure 2 the NIKA/30 m measured beam pattern compared to the best fitting model to the data of K13 (blue). For practical purpose we have divided the beam on three regions : short angular scales corresponding to the main beam, intermediate angular scales corresponding to the first error beam, and large angular scales assimilated to far side lobes of the 30 m telescope. To compute the main beam we have performed a Gaussian fit to the full observed NIKA beam pattern. The best Gaussian fit is shown in yellow. The NIKA main beam is consistent with the one of K13 as we expect the main beam to be defined by the diameter of the 30 m telescope. In the same way far side lobes, which are expected to come mainly from the second and third errorbeams of the 30 m telescope are also consistent between NIKA and K13. However the first error beam observed in the NIKA beam pattern is larger compared to the one modeled by K13. This is not unexpected as the observations were carried out at a different time of the day, under different weather conditions.

This figure is presented in a companion paper : Catalano et al. 2014, 2014arXiv1402.0260C and thus we prefer no to included in this paper.

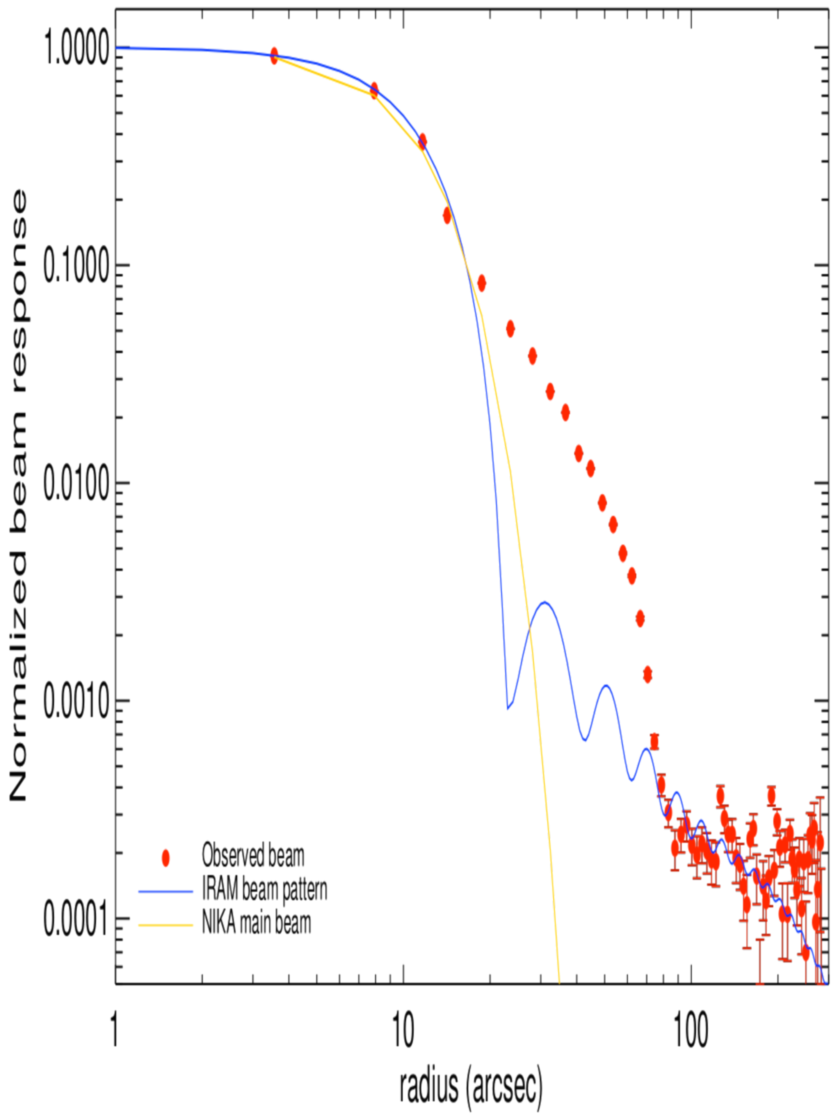

Figure 2: Measured NIKA/30 m beam pattern (red dots) compared to the K13 beam pattern (blue solid line), and to the best-fit Gaussian model to the NIKA data (yellow line).

Thermal Sunyaev-Zel’dovich dedicated data analysis and map-making
----
4.1

Equations (1) and (2) express the SZ effect as a change in Tcmb, but this entire paper expresses results in units of specific intensity. Replace equations (1) and (2) with the corresponding equations for g(x) and delta I/I (ex. Carlstrom, Holder and Reese 2002 equations (3) and (4)) for consistency.
***** **Corrected**

"As the expected tSZ signal is small (up to y ∼ 10−3)" All of your power spectra and timestreams are given in Jy, it is much more useful to convert your y estimate for the SZ signal to Jy in this context.
***** **We now give ~10 mJy/beam for the tSZ signal.**

4.2.4

"we use the 240 GHz data, in which the tSZ signal is negligible, to build the atmosphere template" The quantification of this assumption needs to be elaborated upon. Naively, if you take the frequency dependence for tSZ in specific intensity g(x) at just the band centers, the ratio g(240GHz)/g(140GHz) = -0.4186. How can a 42% effect be considered negligible? Two paragraphs later you have the sentence: "Notice that the positive bias introduced in the 140 GHz data by the tSZ signal present at 240 GHz, already smaller by a factor of ∼ 6 (Eq. 1 and 2), is reduced by a factor of 5 when scaled down to 140 GHz and is therefore negligible." Does this factor of 6 come from a full integration of the bandpasses shown in Figure 2 with the SZ spectrum? Please elaborate on the calculation used to get this factor of 6, as I mentioned above, naive calculations with g(x) are much more severe. This entire discussion should be moved up to the first paragraph of this section where the assumption of negligible signal at 240GHz is stated.
***** **The ratio g(240GHz)/g(140GHz) is indeed about 0.4. Integrating over the bandpasses does not change this number significantly. However, we need to account also for the beam dilution that**
is about a factor 2.25. This is why we obtain a factor of about 5.5
We have also included in the main text a reference to section 4.1 where the unit conversion factors are given.

You address the kSZ as a contaminant, but should also include a discussion of the difference in signal from point sources between the two bands. At 1.1mm the presence of dusty submm galaxies with steep spectra is much more prominent. The contribution is likely to be small, but should be quantified. The signal from radio sources such as the one at the center of RXJ1347 will be affected differently.
***** **This paragraph has been completely reorganized accordingly**

4.2.5

"Frequency lines (e.g. at ∼ 6 Hz) are induced by the pulse tube of the cryostat and observed in the TOD power spectra (see Fig. 3, right panel). They are flagged and set to zero." Is this flagging being done in Fourier space? If you aggressively notch filter with a function that is simply 0 at 6Hz and 1 elsewhere, and your scan rate is 15 arcsec Hz, there is bound to be some ringing induced in the reconstructed signal. Do you see

negative lobes in a map around a bright point source? How deep are they? In General this filtering has a non-negligible effect on reconstructed signal, this becomes very evident later from your simulations, in particular figure 5. Including a plot of your transfer function would be helpful to understand these effects and your measurements throughout this paper.
***** **The flagging of the pulse tube lines is performed in Fourier space, and only for frequencies above 1 Hz. Simulations have proved that these lines have a negligible impact on the reconstructed signal. The lines are all above 1 Hz and they correspond to scales that are smaller than 15 arcsec (notice that the scan speed is 15 arcsec/sec), i.e. smaller than the beam. We do not observe any filtering effects on bright point sources. However as you indicate, according to simulations, we have some filtering effects at larger angular scales (see below).**

4.4

This point source needs more attention. Interferometric results by Plagge et al were able to remove the point source by its structure in the uv plane and concluded that the peak in SZ signal was coincident with the X-ray peak and that the SE extension was a secondary peak. Mason et al did a map domain source subtraction using the Pointecouateau et al flux level and produced a map that had the SZ peak in the SE. Your measurements measure the absolute SZ peak in the SE but are subject to model dependent source subtraction. You must address this discrepancy, if not here then later in your results discussion. How large of an error in the point source amplitude would you need to move the SZ absolute peak to the X-ray centroid?
***** **This issue is now discussed in details in Section 7.5, Comparison to other external data sets. We would need more than a 5 sigma (5 * 0.3 mJy) uncertainty on the amplitude of the source to move the maximum decrement to the Xray peak.**

5.1.3

Photon noise is not "intrinsic" to the detectors, it is real measured power and represents the fundamental lower noise your system can achieve.
"It is supposed to be independent of the observing conditions" - Do not use "supposed to be", this is not scientific phrasing. Clarify the wording of the footnote. The photon noise can be calculated for a given optical load and your optical throughput, it is not clear if that is being included in your simulations.
***** **We actually meant uncorrelated noise rather than intrinsic detector noise. We simply simulate an overall white noise contribution accounting for various sources including photon noise, spontaneous Cooper pair breaking due to thermal noise fluctuations and electronic white noise. This has been clarified in the text. We do not account for variations on the optical load in the simulations.**

5.1.6

It would be useful to include a central point source in this simulated cluster as well as per my comments on section 4.4.
***** **This is discussed in a new paragraph of section 5.1.7**

5.1.7

"We also show the profiles recovered after projection only," Expand on what the "projection only" profiles are. When you say "projection only" does that mean there is no filtering done?
***** **Yes, we simply project the input signal data without decorrelation or filtering. We only account here for projection effects.**

"First of all, due to the scanning strategy, ... annuli"
As I mentioned in my comments on 4.2.5, this paragraph would greatly benefit from a figure which shows the angular transfer function, analogous to Figure 2 in Korngut et al 2011.

***** **Because of our particularly not optimal scan strategy and the fact that the reduction is expected to improve in the future with better 240GHz data, we prefer not to include a figure of the transfer function. However it has been estimated and it is provided in the figure below. In the paper, we have better use the simulations to clarify this issue.**

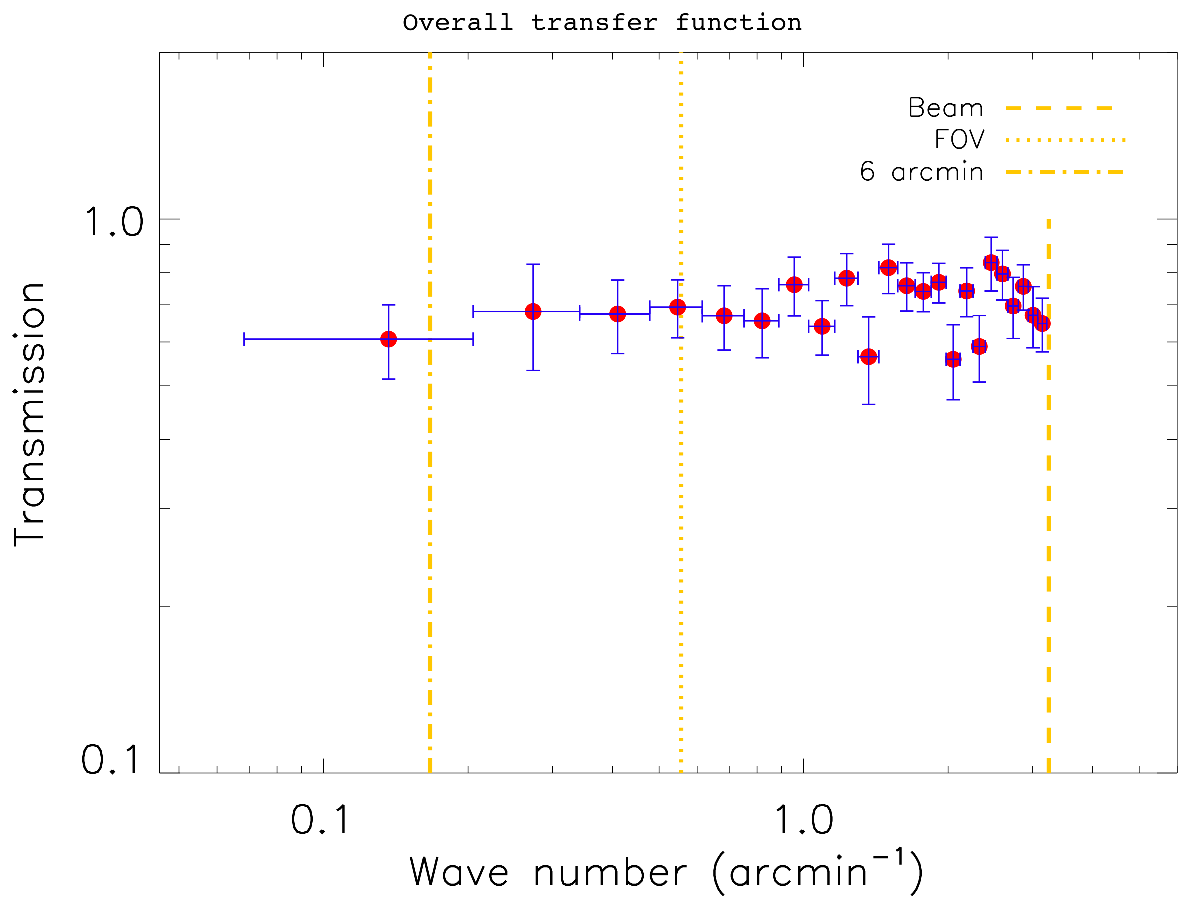

Fig 3. Transfer function obtained from the simulations of the compact cluster case. For illustration we show in vertical lines the wave number corresponding to 6 arcmin scales, to the size of the FOV and to beam pattern.

The "validation" of your pipeline is only halfway complete. You give the general form of the parametrized cluster profiles you used as inputs, but do not show how the values you assigned to those parameters for the two cluster types are affected by your reconstruction algorithm.
"The shape of the profile is not affected in the case of the compact cluster" - Quantify this. Comparing the red datapoints to the solid red line in Figure 5 makes me reluctant to agree with your statement.
Table 4 needs 4 more rows which give the same parameters after fitting the gNFW to the reconstructed images. If your claims are accurate, the numbers for the Compact cluster should be unchanged. Is this 1%? 10%? The parameters will certainly be affected in the case of the "Diffuse" cluster.

***** **In order to do what you propose, we decided to use the same gNFW slope parameters, alpha beta and gamma, for both the simulation and the profile used for real data fitting. We have also changed the two clusters. The diffuse one is broader, but not as much as what we did for the previous version of the paper. At the time the diffuse cluster was for illustration purposes only (way much larger than RXJ1347) and we did not want to give fit parameters on such a cluster that was not realistic at all.**

Figure 4.

Include 2 more panels which show the input maps on matched color scales. Showing the recovered maps alone is not useful.

***** **We have done as you request**

5.2

"This target has been chosen in order to verify that the tSZ signal observed in RX J1347.5-1145 data is not due to a bias in the analysis." I agree that mapping a field without SZ signal present is an important null test for the observation and data processing pipeline. However, why then would you choose a field with a known (albeit faint) cluster present? Surely this test would be better suited to a field with deep ancillary coverage and no known cluster present. The result of the test is null, validating the pipeline, however if something were detected toward the cluster how could you distinguish between algorithm artifact and SZ signal? Reword this section with this in mind.

***** **For the NIKA Run 5 campaign the** IDCS J1426.5+3508 **observations are those that suit the better our purpose of performing a null test. Unfortunately, we did not plan for the observations of an empty field. The text has been corrected following your recommendations.**

6.1

Figure 7: This figure should have 2 panels showing the map before and after point source subtraction with matched color scales.

***** **The figure has been modified in consequence, although the same figure was given a bit later in the ppaer**

Figure 8: The contours in the difference map should be more than just +-1sigma. Matching the colorscale by eye, it appears that there are deviations close to 4 sigma, located preferentially near where the SZ signal is strongest, indicating the presence of uncontrolled systematics. If that is indeed the case this should be addressed. You mention there are differences in uniformity of the noise distribution due to uneven integration time. If this is a significant effect, the pixel histogram in the right should be computed separately for the two regions and the difference in sigma quantified.

***** **As you indicate the noise map is not homogeneous and it is not minimal at the tSZ peak. There is no hint of uncontrolled systematics. What you quote as 4 sigma is in fact ~4/1.5 = 2.6 sigma. For illustration we present below the standard deviation map that we obtain. Inhomogeneity on the noise is now directly addressed on the main text and on the caption of the figure.**

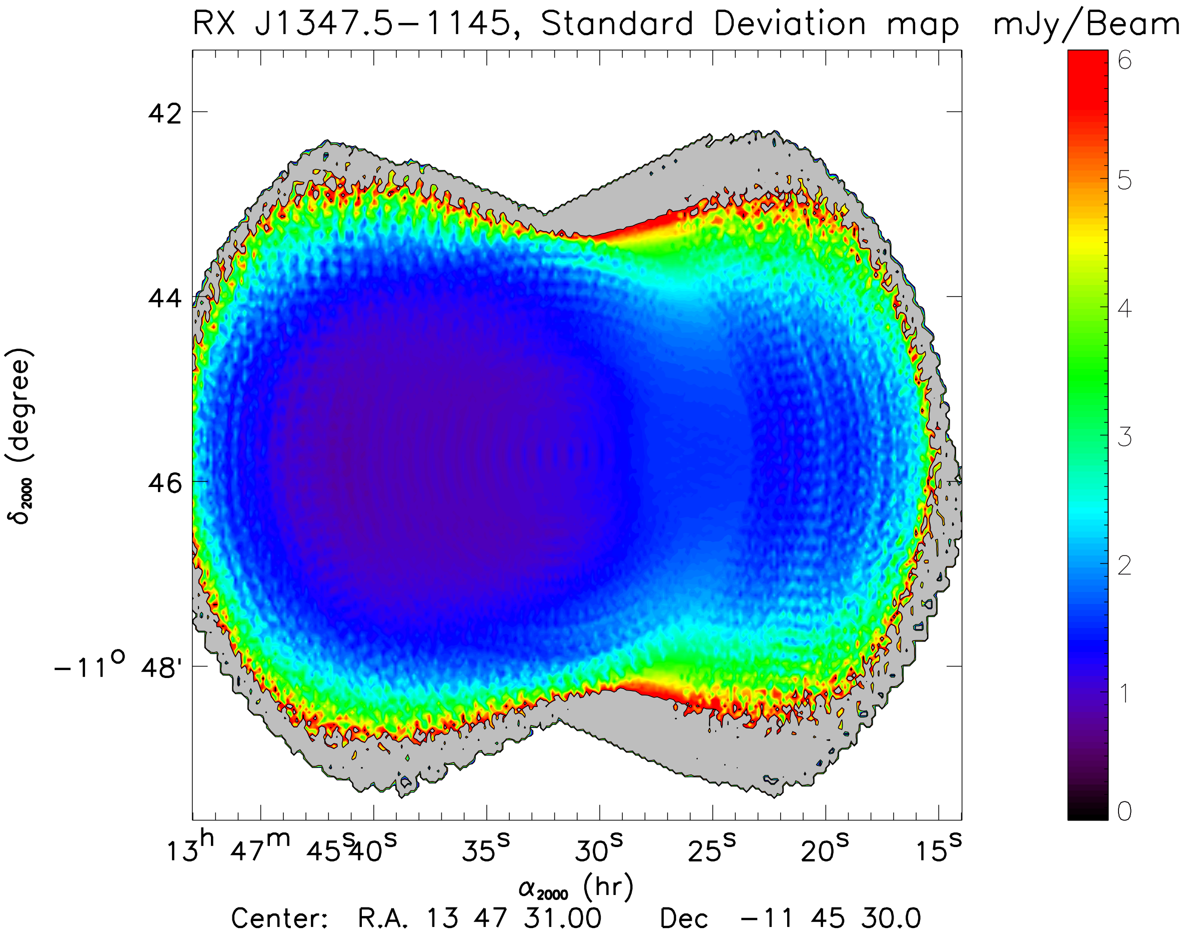

Fig 4. Map of the standard deviation per pixel for the RXJ1347.5-1145 observations.

"The signal is extended and does not coincide with the X-ray center, (R.A, Dec) = (13h 47m 30.59s, -11o 45′ 10.1″), as expected from the merger (see Sect. 2). The tSZ maximum corresponds to the shock location,"
This is just plain wrong, and indicates that you have not read the previous literature in depth. The location of the absolute maxima agree in the CARMA measurement. Here is a quote from the Results section of Plagge et al: "In contrast with previous work (Mason et al, Komatsu et al), we find that the peaks of the SZ and X-ray signals are coincident; the peak is likely suppressed in the single dish maps by the point source emission." You have to address this.
******* We have now included a reference to the CARMA results and discuss the discrepancy both in Section 6 and 7.**

"The shock contribution to the tSZ signal is estimated to be 37% within the 2 arcmin radius." This measurement was done in both Plagge et al and Mason et al with extremely different conclusions. Here are the relevant quotes from Mason and Plagge: Plagge et al: "We find that roughly 9% of this cluster's thermal energy is associated with sub-arcminute-scale structure imparted by a merger"
Mason et al.: "Compared with the bulk emission component, which are also convolved with a 1 beam, the small-scale features are a 10% effect." Mason and Plagge find the substructure to be a 10% effect and you get 37%. Furthermore, all previous SZ studies find the excess isolated to the SE of the cluster center and the NIKA map is extended all over the south, as shown in the right panel of Figure 11. Address these discrepancies.
******* First of all we want to stress that this percentage was derived by considering the cluster innermost region (2 arcmin), where the shock contribution is still important with respect to the total SZ flux**

(integrated over the same area). However, the way we performed this calculation was misleading. In fact, the flux associated to the shock was calculated in the same circular region (centred on the X-ray centre) used to compute the total flux on the NIKA map. This approach brought us to include within the shock-associated flux also some correlated noise fluctuations, not properly accounted for in the error bars. This happens where the signal to noise is low, in the north-west region. In the present version of the paper, we provide error bars that have been calculated by considering the variance of the map. The information about the additional uncertainty introduced by systematics is given as an estimate of its relative contribution (19%), while the model statistical uncertainties have been neglected. In addition, we now integrate the shock residual over a region corresponding to the area we masked when looking for the best-fitting P0-rc with the MCMC code. This leads to Y_shock=0.52 x 10^{-3} arcmin2. This flux implies a shock contribution of the order of 30% with respect to the SZ signal integrated within 2arcmin, which goes down to 24% when considering the Planck Y5r500. This has been clarified on the text. Comparison with the percentages given by Plagge and Mason is quite hard and not necessarily meaningful. Plagge et al. determine that about "9% of the SZ signal is localised in the disturbed region of the cluster" by comparing their data to the X-ray measurement of Allen et al. (2008). "To describe the bulk SZE cluster emission" Mason et al. "construct a simple empirical model assuming the - Xray based - isothermal beta-model of Schindler et al. (1997) normalized by SZE measurements of Reese et al. (2002) and Kitayama et al. (2004)". However, if we consider the error bars of our fluxes as well as the differences of the approaches adopted in the other papers, in our opinion there is no significant discrepancy.

6.3

"Consequently, the flux relativistic correction (Itoh et al. 1998) is estimated to be 7% for the channel at 140 GHz, and it is thus neglected." Is this accounted for in the systematic error values quoted on P_{o} and Theta_{s}? If you calculated the relativistic corrections to quantify it, why not include them in the fit? I would not call a 7% effect insignificant.

***** **Relativistic corrections are not accounted for in the systematic errors given. We intended to warn the reader that for temperatures like those of this cluster, relativistic corrections are not insignificant, as you indicate. However, the electron temperature distribution of RXJ1347-1145 is not trivial, and not uniform either. More especially if we consider the scales explored and resolved by NIKA. We therefore preferred to provide upper limits, since detailing it further would need approximations that would not necessarily lead to more accurate conclusions. The aim of the present paper is to characterize and validate the first SZ NIKA map. Since we are not deriving constraints on the physical properties of the considered cluster and the estimated corrections are however smaller than associated errors, we decided to treat relativistic corrections this way. To avoid confusion the sentence indicating they can be neglected has been removed and replaced by one indicating the necessity to propagate them. In the future we will include them in the cluster modelling, by properly accounting also for the further degeneracies they do introduce.**

Furthermore, you will note that the best fitting values for P0 and rs reported in this paragraph are slightly different from those of the first version of the paper. This is due to a bug in the MCMC code (wrong beam convolution of the model proposed by the chains while exploring the parameter space).

7.1

"On the one hand" and "On the other hand" This figure of speech is not appropriate for this context.
***** **It has been removed.**

"We can then conclude that, despite the large angular scale cutoff (above 3 arcmin), NIKA is able to recover most of the tSZ signal"
You should tie this into your simulations described earlier and explain how RXJ1347 compares to your "compact" and "diffuse" clusters mentioned earlier. If you include a measurement of your transfer function you should be able to quantify how much flux has been filtered out.
***** **We have added a sentence discussing the compact cluster results, which is very similar to RXJ1347 with regard to radial extension.**

7.2

"The data reduction and the instrumental similarities with NIKA make it a perfect choice for a direct comparison." Are the angular transfer functions comparable? Once again please remove the word "perfect".
***** **Indeed we used similar analysis according to the similarities between NIKA and Diabolo (frequency band and telescope)**

"In both cases, the tSZ maximum is not located at the X-ray center, as expected." These two maps rely on the same flux estimate for the central point source, so it is a systematic common to both measurements. Once again you need to address the discrepancy to Plagge et al with the location of the absolute maximum.
***** **We did so**

"XMM data (Jansen et al. 2001) have been used to compute a number count map of RX J1347.5-1145 that we compare to the NIKA tSZ observations."
What do you mean by a "number count" map? Do you mean photon counts? Has this map not been exposure corrected to convey an X-ray flux? Why not use the calibrated Chandra map?
***** **Yes, we meant photon counts and we have corrected the text. We have chosen the XMM map since it was produced by one of our colleagues and thus we have a better handle of the processing performed on this map. The map has been exposure corrected.**
For the XMM data, Jansen et al is not the right citation, Gitti et al use this map in the right context.
***** **Corrected, Gitti et al. 2004 is cited**

7.3

"Indeed, the map of Fig. 13 shows that the NIKA prototype is able to probe the ICM at larger radii than X-ray data."
I do not agree with this conclusion based on Figure 13. The red contours extend to the end of the NIKA coverage and this statement is not quantified in any way. While X-ray exposures are expensive, they are not subject to the filtering of any angular scales. Remove this sentence or quantify its contents.
***** **The sentence has been removed**