Dear Mike,

Thanks a lot for your comments, which surely helped to improve this paper. The comments in the attached pdf were applied, and please find bolow the answer to your major comments.
Regards.
Rémi

Hi Remi - please find below several major comments on the paper, and attached a marked up pdf trying to fix the grammar etc for you (you'll have to open in adobe reader to see them properly). I'm looking forward to seeing the next version - cheers, Mike

1) I really worry about the way the astrophysical contamination is handled here. It seems as though the CMB and CIB are treated as random errors that only contribute image-space variance. This isn't a really fair treatment, and why we bent over backwards in previous work to model the true sky emission as best we could. I think it would be much better to vary the CMB in the actual field as measured by say Planck (and passed through your full transfer function), and the CIB as modeled by me (or use the real images if you prefer), and vary those models about their allowed uncertainties. The main problem here is that the true sky is extremely covariant, and I don't see how that's taken into account in this analysis.

Concerning the CMB:
It is not possible to extrapolate the CMB, measured by Planck, at scales larger than ~10', to the 2 arcmin NIKA field of view. If we pass the Planck CMB map in our transfer function, we get zero (small scale smoothing from Planck and large scale smoothing from NIKA).
Using CMB simulation from LCDM power spectrum obtained with CAMB, we have checked that CMB fluctuations are negligible for us. We expect the CMB to show up at 7 and 15 uJy/beam rms in NIKA 260 and 150 GHz bands in a 5 arcmin FOV.
In fact, the CMB is neglected and not accounted for as a random error, even if it would certainly be the only way to account for if it was significant.

Concerning the CIB:
1. Sources that are directly detected by Herschel are taken into account by modeling their SED and extrapolating them to the NIKA bands. This is what is described in section 3.3. These sources are listed in table 4 with their extrapolated fluxes. The corresponding template is given in figure 2. For this work, we use the SPIRE catalog that you provided us.

2. Only a small fraction of the CIB is resolved by Herschel. This is why we produce CIB realizations as described in section 3.1, but we stress that this is in addition to the Herschel detected sources that are subtracted separately. At the scales probed by NIKA, the clustering term is subdominant with respect to the shot noise arrising from unresolved sources bellow the detection limit. The shot noise power spectrum is flat, and therefore, it does not introduce spatial correlations. It introduces correlations between the two NIKA bands but they are accounted for. You can see this in the power spectrum below with the transfer function in red, the shot noise as a constant level, and the clustering as the spectrum decreasing with k.
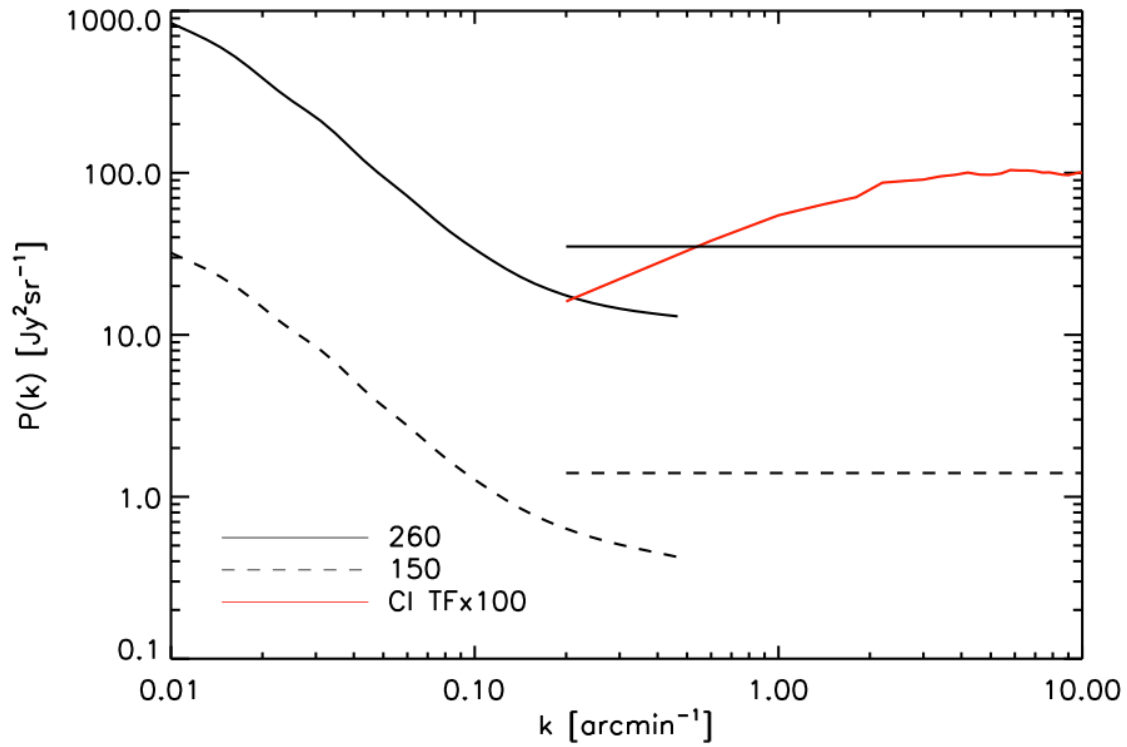
As far as we understand, what we did is in fact very similar to what was done in Sayers et al. 2013, so your comment is not clear to us and maybe we did not understand it.
Then, how can you produce a CIB template? Do you mean a template for Herschel detected sources? In this case, this is what we have done. Or you mean a tempalte for the confusion noise? In this case, you need to extrapolate it to the NIKA bands, account for tSZ, kSZ and other potential sources of signal. How can you do this? This sounds very risky, in particular at the scales probed by NIKA. Any local mismodeling can show up as fake signal. In particular, if the SZ signal is mis-accounted for, this could appear in the CIB template and be removed/added in NIKA data.
Maybe we did not understand your point?

2) Section 5: I'm not understanding why you're modeling the tSZ, subtracting it, and then modeling the kSZ. Looking at equation 6 and 7, I don't see how the v_pec term in g(stuff) drops out of the equation. Are you assuming that v_pec =0 in your derivation? I would say the more canonical approach is to do a joint fit for y and v_pec. Isn't that a more fair treatment? What am I missing?

There is no model invoved in Section 5. We are just inverting equation 3 based on the two band measurement. The velocity does not appear in g(stuff) because velocity induced relativistic correction are negligible. Then we extract y_tSZ and y_kSZ and y_kSZ, which are given by eq 4 and 5 (i.e. related to tau, v_z and T_e).

3) Looking at the very negative velocity for clump B (eg Figure), I do worry that this might be an artifact of the astrophysical sky + noise + your transfer function modulating it. As in point (1) above, propagating the measured sky through your transfer function rather than performing random realizations and treating it as a noise may uncover some features you don't expect. I think a figure showing the full end-to-end transfer function of the measurement would be a good addition.

This has been deeply investigated and the fact that the velocity of B can take very high values (in case F1) only reflects that we are limited by degeneracies between density and velocity. This is why we also test using an Xray prior on n_e (i.e. fit F2).

The plot of the transfer function has been shown in previous work. We do not want to add an extra plot. You can see the transfer function here:
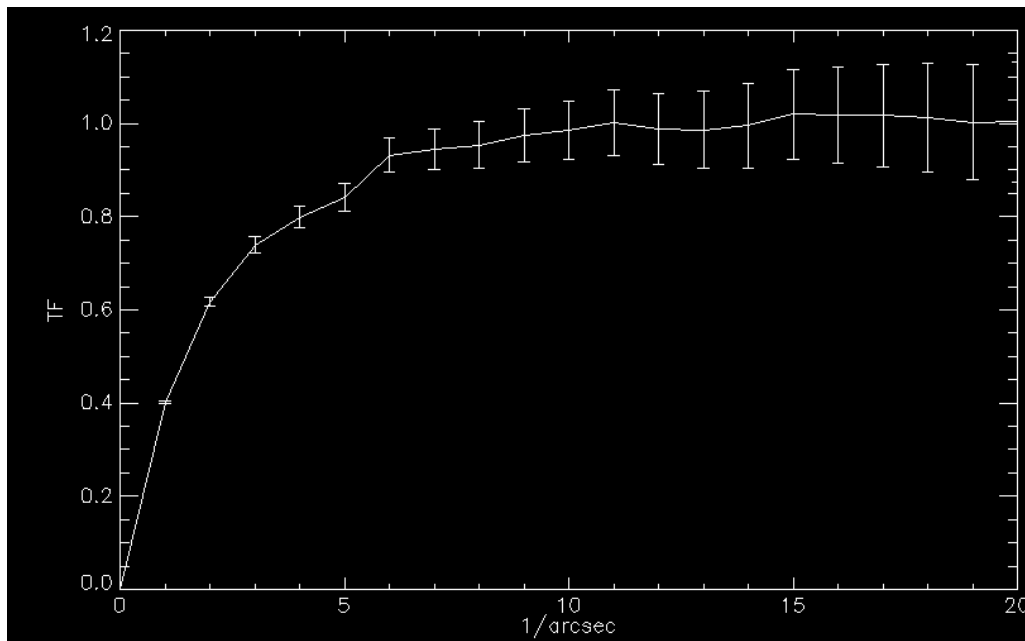
4) In Section 6, there is a great deal of discussion of the velocity model, but I'm having a lot of trouble sorting out what are observations about the best fit model, and what are observations about the residual of the model and the data. I would suggest reducing or removing any discussion about the structures in the residual map, since they depend on a lot of errors which seem as though they are difficult to quantify, and anyway none of the residual map structure is significant.

5) There is a great deal of repetition and extra verbiage in this paper. I would imagine it could be shortened by 30% just by applying a thorough hand during editing.

To comments 4 and 5: we fully agree and this is probably in part the results of previous things added uncoherently from different co-authors. We have worked on reducing/removing/clarifying the text, which should be easier to follow now. The introduction has also been reduced.
Concerning the residual plots, in fact the contours on figure 7 provide the signal to noise ratio. The model is consistent with noise almost everywhere on the map. We agree that the discussion is a bit speculative, but it was requested by other co-authors, so we reduced it but we did not remove everything.