Moderate requests

1. In Section 7.2 the authors discuss the main result of their study - the lack of a strong correlation between the IR-measured star formation rate (SFR) and AGN X-ray luminosity (Lx). In this section, they compare to the Netzer et al. (2009) and Rosario et al. (2012) studies, yet Stanley et al. (2015) represents a significant update to those earlier studies, so I consider it warrants a comparison in this section. Further, since Stanley15 also accounts for the AGN component to the SED (as done in this study), it also represents a more relevant comparison study. Related, Stanley15 present an update to the Hickox et al. (2014) tracks (see Fig. 7, lower panel, in Stanley15), which are much flatter than the Hickox14 tracks, and therefore more closely match the findings of this paper. Since the authors refer to the Hickox tracks as being based on the upturn of the Rosario findings, a comparison of their flatter SFR-Lx result against these new tracks - even if just qualitative - would be insightful.

Authors’ response: The authors agree that a comparison with Stanley+15 is warranted given the similarities between the samples and analysis. We have added 4 paragraphs starting on page 15 that goes through a simple comparison. We produced a new plot (Figure 12) that qualitatively compares the SFR-LX relationships from our work with those at higher redshift in Stanley+15 and find broad agreement. We comment in the text on how it is likely that the same models that accurately describe the Stanley+15 relationships would describe our local relationship as well.

2. In Section 5, the authors report that they use ASURV to account for the upper limits in their data. In their original paper, however, Feigelson and Nelson (1985) suggest that this technique is only reliable when the upper limits are randomly distributed among the detections. The authors do not discuss whether this is the case for their data. Could the authors please provide this information, possibly in the form of plots showing the distributions of specific luminosities for detected and non-detected AGNs (as upper limits). These plots wouldn't necessarily need to appear in the paper, just sent to me (via or cc'ing MNRAS) would suffice. If they aren't randomly distributed, the authors should explain why the use of ASURV is appropriate in this instance.

Authors’ response: This is a valid concern since one of the main ASURV assumptions is that the upper limits are relatively randomly distributed among the detections. We have plotted the distributions for the detections and upper limits in each waveband and LX bin and ran a 2 sample KS test to determine the probability that they are from the same parent population. All sources used in the analysis are detected in each WISE band so we do not include them. Further there are some waveband/LX bins that do not contain any upper limits so these are not included as well. The LX bins are labeled 1, 2, 3, 4, and 5 that correspond to increasing LX luminosity.

From the plots of the distribution, it is clear that in some cases the assumption of randomly distributed upper limits is not correct. However in most of these cases, the number of upper limits compared to the number of detections is quite small. These are mainly in the 70 and 160 micron bins where our detection rate is very high for our sample. For all of the LX bins at 70 and 160 micron, the KS probability is below 5% meaning the upper limits and detections are not drawn from the same parent population.

To test the effect this could have, we recalculated the median SEDs by not including any upper limits at 70 and 160. We have provided a new Figure 2 for Dr. Mullaney to compare (‘avg\_sed\_binned\_lx\_lnu\_noulPACS.pdf’). The two figures are virtually identical so we are led to conclude that the upper limits in the 70 and 160 bins do not have much, if any, of an effect on our results. To further compare we also provide another version of Figure 2 where do not include any upper limits in any band (‘avg\_sed\_binned\_lx\_lnu\_noulall.pdf’). Here, especially at 500 micron, the effect is quite pronounced by not including upper limits. The KS probability for the longer wavelength bands is greater than 5% validating the assumption that the upper limits and detections are drawn from the same population.

Minor requests (those marked with a \* are merely suggestions):



Introduction, left column, around lines 55/56: Should you wish, you may refer to Harrison et al. (2012) who demonstrated that the conflicting and contradictory findings of some previous studies are due to small number statistics.

Authors’ response: We have added the Harrison+12 reference in the introduction.

Section 3.2, right column, around line 35. Please state how many of the sample are associated with sources in the 2MASS XSC.

Authors’ response: 294 galaxies are in the 2MASS XSC and we have added this to the text.

Section 3.2, right column, around line 51: Presumably you used the profile-fitting magnitudes for increasing numbers of sources from W1 to W4 due to the increasingly large PSF (and thus fewer sources being resolved). If that’s the case, please say so, otherwise please explain why these numbers increase so much.

Authors’ response: Dr. Mullaney is correct that the increasing PSF FWHM led to more sources for which we used the profile fitting magnitudes. We have added clarification of this in the text.

Fig 1 and elsewhere. It is not clear why you prefer to show KDEs rather than the far more commonly used histograms, could you explain this preference? Neither do you state what a Kernel Density Estimate actually is or how you have produced yours. Please rectify this.

Authors’ response: We have added a footnote explaining what KDEs are and why we choose to use them in Section 4.

Section 5, left-hand column, around line 33. Where you state you calculate the median luminosity density in each X-ray luminosity bin, is this for every wavelength? It seems like it is, but it’s not explicitly stated.

Authors’ response: We have added a phrase in the first sentence of Section 5 to explicitly state the median is calculated for every wavelength in every X-ray luminosity bin.

Section 5, left-hand column, around line 60. In this context, when stating that the SFR is unrelated to X-ray luminosity, please note that this is for SFRs on timescales of ~100Myrs. It may be different on shorter timescales (see later point).

Authors’ response: We have added a phrase in Section 5 on page 5 pointing out the SFR timescales for which we are sensitive to.

Figure 2: The extent of the error bars are difficult to see since they overlap so much. I suggest you stagger the bars a little to prevent this overlap.

Authors’ response: We have regenerated Figure 2 so that the median SEDs are offset slightly to be able to discern the error bars better and have updated the caption to explain this.

Section 6.1, page7, right hand column, around line 33/34. Is it really the case that all the parameters of the MBB are unconstrained in some AGNs? Surely the upper limits put some constraints one at least some parameters (an upper limit is, after all, a constraint).

Authors’ response: We have removed the phrase about being unconstrained from the sentence. By unconstrained we only meant that a definitive value couldn’t be found and only upper limits could be estimated for the MBB parameters. But Dr. Mullaney is correct in that upper limits still provide a constraint.

Section 6.2: In many cases, the host galaxy dominates at longer wavelengths, which surely means that the position of the turnover of the warm component (attributed to the AGN) is poorly constrained. Thus, since this AGN component could extend to longer wavelengths, is there a possibility that you are underestimating LAGN? Perhaps make a statement on this point in this subsection.

Authors’ response: This is a good and valid concern. To address it we decided to perform a small test. We downloaded the Symeonidis+16 (S16) AGN template which extends to longer wavelengths than any other AGN template in the literature. The S16 template was used in combination with DecompIR to see how much our various luminosities are affected. We found only a small difference between the AGN IR luminosity using this new template and the ones we calculate. The spread in the difference is of the same order as the uncertainties we place on our luminosities (0.2 dex) so we are confident we are allowing for enough uncertainty in our estimates to account for different models and templates. We detail this test and the results at the end of Section 6.2 on page 8.

\*Section 7.2, right hand column, around line 35: When stating that the flux ratio is correlated with Fagn, you may wish to refer back to Figure 2, as this nicely demonstrates why this is the case.

Authors’ response: We thank Dr. Mullaney for suggesting this and have added a sentence in Section 7.2 on page 10 referring back to Figure 2.

Section 7.2, right-hand column, around line 59: The statement about selecting galaxies above a given colour gives 25, 50, 75 completeness is confusing. Complete to what? Do you mean that above a given cut 50% of all galaxies will be AGNs? What about AGN luminosity? Surely this completeness changes with F\_AGN. I agree that 50% of all BAT AGNs have have colours above the given value, but that already implies a (X-ray flux) selection. Please clarify.

Authors’ response: Dr. Mullaney is correct that “completeness” is not the right term to use in this subsection. Rather we simply wanted to see how well each color would select our non-AGN and AGN sample without regards for selection effects. We have removed any reference to “completeness” and added a sentence at the end of the subsection to clarify that this is only a simple analysis that doesn’t take into account complicating factors.



Section 7.3 Left-hand column, around 33. It’s not clear why you adopt 1/3LMBB.

Authors’ response: We have added a sentence explaining this is because for non-AGN galaxies, this is the ratio between the PL and MBB component and to refer back to Section 6.2.

\*Section 7.4. I thought it would be insightful to show a plot of SFR/MDust vs., say, Lx as this would clearly demonstrate that the sfrs of AGNs were just following what we’d expect for their dust (and therefore presumable, gas) masses. i.e., AGNs are not especially “gas rich” compared to non-AGNs with similar SFRs.

Authors’ response: Based on this suggestion, we instead decided to show SFR vs. Mdust for both the BAT AGN and HRS galaxies. This new figure shows they lie on the same correlation. We also note that some of the BAT AGN seem to be lying above the HRS correlation indicating a higher SFR/Mdust ~ SFR/Mgas = SFE, meaning a higher efficiency for star formation in the BAT AGN and note this has been tentatively seen in previous work.

\*Section 7.4, Page 13, Left column, around line 19: Isn’t this connection between dust mass and SFR likely just a manifestation of the Schmidt-Kennicutt law, since dust mass is linked to gas mass? Later, you make this connection, so you may wish to highlight this here as well.

Authors’ response: Dr. Mullaney is correct about this so we added a phrase referencing the Schmidt-Kennicutt law.

\*Section 7.5, Page, 16, around line 8/9: You could refer here to studies that claim to have observed quenching in outflows from high-luminosity AGNs, such as Cano-Diaz et al. (2012), Cresci et al. (2015), Brusa et al. (2015), Perna et al. (2015).

Authors’ response: Thank you for the suggestion. We have added these references in Section 7.5.

Section 7.5, Page 16, around line 19. Following your discussion about dust mass, I was confused by your reference to AGNs living in gas rich galaxies, but with a delay between the peak of star-formation. Earlier, however, you show that the SFR-Mdust is consistent with star-forming non-AGNs. Thus, assuming dust mass is a proxy for gas mass, then this doesn’t suggest that AGNs have a higher gas to SFR ratio compared to non-AGNs, which is what your interpretation would suggest. Please comment on this.

Authors’ response: Yes this is correct however I am comparing to the relatively gas poor quenched galaxies. AGN occur in galaxies that still have a significant gas reservoir but SF is either ongoing or slightly subsisted. We have removed the term “gas-rich” and chose instead to say galaxies with “on-going star formation”.

Figure 11: It would be useful to show the position of the z~0 “Main Sequence” on this plot, to see how these AGNs compare to non-AGN, star-forming galaxies.

Authors’ response: We have added the z~0 Main Sequence to this Figure.

Appendix A3: "We found this imposing this prior resulted in better and more realistic fits to the SEDs" - compared to what? A Flat Prior?

Authors’ response: Yes compared to a flat prior. We have added this clarification to this Section.

Appendix B: First paragraph: "...which were only detected by Herschel \*in\* less than one waveband..."

Authors’ response: We have corrected this typo.

Appendix B4: "...and DecompIR and D14 models for \* \* L\_IR" (i.e., missing space).

Authors’ response: We have corrected this typo.

Appendix C: "...silicate emission is absent prompting the need for \*our\* own modelling."

Authors’ response: We have corrected this typo.

\*Finally, I think the "abundantly clear" statement at the end of your conclusions is far too strong. I agree that it is increasingly clear that there is little correlation between measured FIR SFRs and Lx, but this may be just one aspect of this relationship. The true relationship could be far more subtle (note: "could", I'm not suggesting "is"). For example, you and others have shown that X-ray AGNs show at least a preference for star-forming galaxies, which implies a relationship at this most basic level. Following recent studies looking at the probabilities of finding an AGN of a given power (e.g., works be Aird, Bongiorno, etc) it may be the case that the \*likelihood\* of a given LX \*is\* linked to global SFR. I only make this as a suggestion, but I worry that such strong a statement (especially as a closing statement) could be seen as naive by future, more nuanced studies.

Authors’ response: We thank Dr. Mullaney for the concern and we agree with this sentiment. We have removed this sentence completely.

Other changes:

Beyond the requests and suggestions by Dr. Mullaney we also have received comments from Dr. Dieter Lutz who suggested several other changes. These are mainly minor and we highlight them in blue. We document the changes below.

1. Include a reference and comparison to Asmus et al 2015 for our MIR-LX relationship. See page 9.
2. A comment on very small grains with high temperatures potentially complicating our measurement of the 70 micron AGN contribution See page 11.
3. A reference to Santini et al 2012 in Section 7.4.
4. A correction to our comparison with Rosario+12 where he pointed out that IRAS undetected sources were not removed from their Swift/BAT sample therefore the differences between the Rosario+12 relationship and ours can’t be due to this.