AUTHORS: We would like to thank the referee for their detailed and useful comments leading us to a careful revision of the work. We took their suggestions into account as much as possible and modified the Manuscript accordingly. Below you can find our response to individual comments. Relevant changes are highlighted in boldface.

Reviewer's Comments

This paper compiles a range of multi-wavelength data for a set of galaxies which have been observed with ALMA at high resolution in order to constrain the CO emission distribution and the nuclear mm-wave continuum. It explores the nature of the nuclear mm-wave continuum using spectral index and SED fits, and concludes that it is synchrotron or IC-dominated. A main part of the discussion examines the evidence for AGN feedback on the molecular gas. In particular, a drop in the concentration of circumnuclear molecular gas, reported by earlier ALMA studies, is not supported by this work.

Since this is an independent assessment of recent work in an active field, I would consider it suitable for publication in MNRAS. However, there are a number of weaknesses in the current approach to the work, which need to be addressed before it can be published. In addition, the paper lacks rigour in places, and a tightening up of the arguments, along with a better discussion of the ancillary measurements and analysis, is required.

The paper could be better written and presented. While its structure is fine, there are numerous grammatical or sentence construction errors which suggest that pre-submission internal editing of the paper was somewhat cursory.

I lay out my major concerns here, and then a list of less major comments which should also be adressed before resubmission. Finally, a non-exhaustive list of typographical errors is placed at the end of the report.

Major points:

REFEREE: 1) The crux of Section 5 is the relationships between AGN power and molecular gas mass, and their comparison to other works. I'll bring up my worries about the estimates of AGN power in the next comment, but I'll lay out another concern here. The paper suggests several times that the sample used here is not biased by selection based on AGN properties or luminosity, which is a fair point. However, there is a major bias in the original selection, which could have major effects on the conclusions. This is the selection basis for the WISDOM survey, a set of galaxies where the SMBH sphere of influence extends to circum-nuclear scales resolvable by ALMA. This is what drives the drastic difference in SMBH mass between the samples of GB+21 and this paper (Figure 2). While this isn't shown (and probably should be in an updated draft), I'd predict the bulge mass, bulge prominence, and central velocity dispersions of the galaxies in this work are also quite different from those of GB+21. The nature of the central potential and the stabilising influence of the bulge on molecular gas dynamics is well-known, and, in the absence of any evidence to the contrary, one must conclude that this is the primary reason that this study finds generally lower central molecular gas concentrations than GB+21.

In addition, the sample from this paper has minimal coverage at LX(2-10) > 10^41 erg/s, which is where most of the GB+21 sample lie and where the drop-off in concentration of CO has been reported.

Putting these two points together, I am not convinced there is ample evidence for a different conclusion to GB+21. The paper currently does note that secular processes can play a role, but does not fold in the consequences of this (much more likely) explanation into the interpretation or presentation of Figure 9. For e.g., if gas velocity dispersion or shear can lower the baseline gas concentration in the centre of the WISDOM sample, it would be beneficial to indicate the degree by which this may lower the concentration compared to a sample with lower central velocity dispersion or shear (i.e., the Seyferts of GB+21). The simpler explanation should be rigorously tested before putting forward a loose explanation that invokes activity timescales that are rather poorly constrained.

Unless it can be clearly demonstrated that other processes can be ruled out - processes which may not be related to the direct effects of AGN on molecular gas - I think the conclusions currently drawn from Figure 9 are subject to some doubt. There is information in the literature to help mitigate this doubt, and the authors should take it upon themselves to bolster their arguments accordingly.

AUTHORS: We thank the referee for this comment. We have decided to present both the AGN impact and secular processes as equal possibilities in what sets the nuclear molecular gas concentration. We have also included a histogram (Fourth panel of Figure 2) of the central velocity dispersions of our galaxies compared to the Garcia-Burillo+21 sample and found that our sample spans a wider range of velocity dispersions. Unfortunately, for the WISDOM sample we do not currently have the bulge masses to compare to the Garcia-Burillo sample.

REFEREE: 2) The determination of the AGN power is rather loose, in particular the reliability of the X-ray and emission line tracers at low luminosities. The reliance on indirect estimates to calculate and correct for contamination is debatable in many cases, and certainly the quoted uncertainties will not reflect the true scatter in these estimates. Indeed, as I've outlined below, there are reasons to believe that the AGN X-ray luminosity estimates for galaxies with stated Lx < 10^39 erg/s could be overstated. Potentially, the use of higher resolution X-ray data (Chandra, XMM) could help with this. If these have been used, the paper should summarise which of the measurements are more reliable because they are based on higher quality data, and which are not. Results should be tested with the subset with the most reliable AGN power estimates whenever possible, and these tests should be folded into the discussion.

I've outlined a number of issues below in the general comments. I'd recommend that the authors work towards a more robust set of AGN power estimates, including a fair assessment of the real uncertainties from which their estimates may suffer.

AUTHORS: We thank the referee for this comment. We have tried to gather the most high resolution data for the X-ray luminosities and have used X-ray data from Bi et al 2020 where the emission detected in only nuclear emission. We have tried to outline as explicitly as possible that the data used in this study could be subject to contamination from other sources and this could be more important at low luminosities. We have additionally looked at our analysis using and X-ray good sample (e.g. only using data taken from Chandra or XMM-Newton) and we find the lack of correlation persists.

Detailed comments:

REFEREE: 1) Sec 1, Para 1, Line 1: The existence of central SMBHs hasn't been established for all galaxies with mass > 10^9 Msun. Consider the Large Magellanic Cloud as an example. It's mass is ~ 10^10 Msun, but there is no clear evidence of a SMBH in this galaxy. Please correct for accuracy and provvide references. Alternately, the authors can easily skip this general sentence and proceed with the referenced statements that follow it.

AUTHORS: We have clarified that almost all massive galaxies have a SMBH.

REFEREE: 2) Sec 1, Para 3: The discussion of host galaxy demographics and their relation to AGN types. The quoted mass ranges with respect to widely used rules of thumb seem to be a bit off. Very massive galaxies are noted as being > 10^10 Msun, but this is almost an order of magnitude lower than the turn-over in the stellar mass function of galaxies in the local Universe. There are, therefore, a large number of galaxies locally which are more massive than 10^10 Msun, including our own Galaxy and the Andromeda galaxy. Most astronomers wouldn't call our Galaxy very massive. A more representative LERG host, M81, is almost 10^12 Msun. This, to most, is a more representative "very massive" galaxy.

Please consider the terminology used here carefully and edit as necessary. This probably won't matter for the science content of the paper, but its important to ensure that statements in the introduction are aligned with the majority opinion in the community.

AUTHORS: We have changed the text to say massive instead of very massive and have updated the mass quoted in the text.

REFEREE: - Sec 1, Para 4: Quoted range of X-ray luminosities is missing units.

AUTHORS: Fixed

REFEREE: - Sec 1, Para 4: "broader distributions of Seyferts." As "Seyfert" is a purely descriptive termand not quantified, it doesn't make sense to talk about a distribution of Seyferts. A better statement would be "among Seyferts with a broader range of properties." or "among the general population of Seyferts."

AUTHORS: We have edited the text.

REFEREE: - Sec 2.2.1: "line channels were ... made." Edit this line for grammar and punctuation.

AUTHORS: We have edited this line.

REFEREE: Sec 2.3: " This figure shows ... tracing nuclear activity." With such a large range of distances in the sample, and the reliance on generally shallow ancillary data surveys, one should be very careful about interpreting correlations between two luminosities, as these can be driven by selection effects.

One would be hard-pressed to attribute Lx as low as 10^38 erg/s to pure emission from an AGN, especially in massive galaxies. This point is even more important if X-ray luminosities have been extrapolated for lower energy bands which have even higher levels of contamination from hot gas or other galaxy components.

AUTHORS: We have tried to explicitly state that there could be contamination from other sources and that the emission shown in this plot may not be exclusively from AGN.

REFEREE: Sec 2.3.1, para 1: "inverse up-scattering" should be Inverse Compton up-scattering.

AUTHORS: We have edited the text.

REFEREE: Sec 2.3.1, para 2: The CGM emission may not be a concern for the brighter X-ray sources, but it could be for the faint end of the targets. This is more of a concern if extrapolations of the 2-10 keV luminosity were made from lower energy measurements where the thermal CGM can dominate.

AUTHORS: We have explicitly stated that the CGM could contaminate the

X-ray emission at lower luminosity and this effect could be more influential in galaxies where the X-ray luminosities has been extrapolated from a lower energy band.

REFEREE: Sec 2.3.1, para 3: The correlations used to estimate the stellar X-ray contributions are valid only for the low-mass X-ray binary population. If there is sustantial star-formation in the galaxies studied here, then the high-mass X-ray binary population should also be considered, in addition to the LMXB contribution.

It important to note that, from Kim & Fabbiano (2004), both the Milky Way and M31 have X-ray luminosities of > 10^39 erg/s though neither of them has a substantially accreting SMBH. This suggests to me that the authors have taken a rather liberal stance with regards the influence of non-AGN contamination on the X-ray luminosities. Please develop a more careful assessment of the contamination to the X-ray luminosity estimates, both from the CGM, from the X-ray binary population, and other potential sources of contamination (background sources, etc).

AUTHORS: We have quantified the contribution from HMXB using a relation between the X-ray luminosity and SFR. We found that in some sources the contribution could be significant. We have explicitly stated which sources could have larger contributions from HMXB. We have looked into the LMXB contribution again and have used and additional relation and have still found the contribution be minimal in some galaxies and explicitly stated in which sources the contamination from LMXB could be larger.

REFEREE: Sec 2.3.2, para 2: "we ensure that our 1.4 GHz measurements encompass any associated emission." This is not a trivial exercise due to the complexity and dynamic range of radio emission in galaxies. Please give more details, because this is an imporatant part of understanding the radio properties of the sample.

AUTHORS: We have clarified that the radio surveys from which the radio data was gathered from have very large beams. This ensures no emission is resolved out and any extended radio emission (e.g. from a radio jet) is included.

REFEREE: Sec 2.3.2, para 2: please quantify "significantly larger than unity". Ideally this should be optimised with respect to the known scatter of the SFR-radio relationship.

AUTHORS: We have clarified this by saying significantly larger than zero.

REFEREE: The term "L1.4, AGN" is confusing, as this suggests a radio luminosity emitted by the AGN. However, the quantity is not a luminosity, but a ratio of the multiplicative excess in factors of L1.4,SF. One may have naively expected L1.4, AGN = L1.4 - L1.4,SF, a subtraction of the known luminosities. I recommend using a clearer term.

AUTHORS: We have changed the term used in the text to a radio excess factor E_1.4.

REFEREE: Sec 2.3.3: [O III] can be completely dominated by stars even in nearby galaxies. The key information is whether the [O III]/Hbeta ratio is high, ideally with the addition of information coming from other lines to account for other systematics. By itself, [O III] is not a sure-fire measure of the AGN power. Also, it is affected by ISM dust extinction which can be severe.

Which HST datasets were used for a measure of the total [O III] luminosity? Is there STIS or COS spectroscopy for the majority of the galaxies in the sample? If so, appropriate references are needed or a description of how these data were processed and lines measured. It seems unlikely to me that HST provides a uniform or complete survey of [O III]. If other datasets were used, they should be listed here.

AUTHORS: We have gathered Beta data where available and computed the [OIII]/Hbeta ratio and found it to be high in most of our galaxies. We have stated that the [OIII] line could be contaminated from other sources. We have doubled check the source of the [OIII] line and found that actually the data was observed using other facilities instead of HST.

REFEREE: Sec 2.3.5: Please indicate which galaxies had their masses estimated using the scaling relation to K-band luminosity from Equation 5. What exact 2MASS catalogue products were used? Were any corrections needed for K-band emission from the AGN?

In Table 1, there are a few objects without listed masses. If the all-sky 2MASS catalogue was used, and knowing that K-band magnitudes are available for all galaxies in Table A1, why are masses not reported for these targets?

AUTHORS: We have indicated the source of the stellar masses in Table 1. We have stated that the 2MASS catalogue was the extended source catalogue. We have clarified that no correction for the K-band emission from the AGN was performed. We have computed the missing stellar masses using the K-band magnitudes.

REFEREE: Sec 3.2: Both X-ray and [O III] luminosity are used to estimate accretion rates, but is isn't clear in which systems one is preferred to the other. Please include a plot of the [O III] vs. X-ray flux and/or luminosity to understand the similarities and degree of scatter between the two measures of the AGN power, and indicate the regimes where one measure of Lbol is preferred to the other.

AUTHORS: We have clarified why we have used both X-ray and [OIII] as a measure of accretion rate as we do not know if our objects are radiative or kinetic mode AGN. We have include the [OIII] vs X-ray luminosity plot in Figure 1 and the bolometric correlation in Figure A1. We cannot indicate what regimes one if preferred other the other. Rather you would use X-ray as the accretion measure in radiative mode AGN and [OIII] in kinetic mode AGN.

REFEREE: Figure 2: This figure suffers from numerous elements of poor presentation and should be extensively worked over. The figure keys use a font that is too small to read clearly. The use of overlapping coloured histograms with transparency leads to multiple colour combinations that are confusing to the reader (even more so if the reader has any amount of color-blindness). Consider using blends of open and filled histograms. Please also highlight the distributions of the sample from this paper more prominently. Yellow is a poor choice of colour because it is too close to white, especially when a transparency of 100% is not used. Please include some summary statistics (medians, KS test p-values) to clearly highlight the differences betwee the distributions being compared. Visual assessment is subject to biases.

AUTHORS: We have update these histograms to made them easier to read and have tried to make it so colour blind people can still read the figure. We have include the medians and KS test p-values in Table A4.

REFEREE: Sec 4.2.3: "excess attributed to SMBH". This should be "attributed to the AGN", in particular the accretion flow, not the SMBH

AUTHORS: We have edited the text

REFEREE: Please clarify with reference to Figure 5 whether this is the total mm continuum luminosity or whether a correction from the SED fits for dust emission has been made.

AUTHORS: We have clarified this in the text.

REFEREE: The "interesting trend" in the last line of Sec 4.2.3 is said to be discussed in Sec 5.2, but I don't think it is really brought up. If it is not discussed, please remove any reference to the trend.

AUTHORS: We have removed this line

REFEREE: Equation 14: There is an error in the mathematics of the inversion for the constant factor. Please check. Only one version of Eqn 13 or 14 is needed, as the inversion itself is straightforward algebra. Preferably Eqn 14 (after correction) as it is the final form used.

AUTHORS: We have corrected the equation.

REFEREE: Sec 5.1: References are made to studies which have reported correlations between molecular gas mass and AGN. However, a lack of a relationship has also been reported in previous studies (e.g., Rosario+ 2018, Garcia-Burillo+ 2021).

AUTHORS: We have additionally made references to Rosario+18 and Garcia-Burollo+21 and the lack of correlations they find in there works.

REFEREE: Sec 5.1.1: A fair amount of the discussion revolves around a comparison to the rather weak correlation reported in Izumi+ (2016).

However, given the different tracers used (HCN vs. CO), which are subject to very different excitation behaviour in XDRs, I think the discussion is not quite balanced. Instead, a better comparison would be to the results of GB+21, who report no correlation between X-ray power and CO gas mass on similar spatial scales to both Izumi+ and this study, while using a molecular tracer that is identical to the one used in this paper. Rather then setting up a tension, it appears to me that the result from the WISDOM sample is consistent with GB+21 and reinforces their result.

AUTHORS: We have included discussion about GB+21 and the lack of correlation that they find.

REFEREE: Figure 9: Place the legend within a box to separate it from the data points themselves.

AUTHORS: We have done this.

REFEREE: Section 6 is best included as a subsection of the Discussion (Section 5), rather than its own independent section. It's not strongly conclusive or breaking new ground compared to previous work.

AUTHORS: We have removed this section as we are working on a separate paper related to this work.

REFEREE: Sec 6, line 1: The line reports a correlation, but Sec 4.2.3 does not. Is this a typo?

AUTHORS: This section has now been removed

REFEREE: Typos or grammar (please search your manuscript to find these):

Sec 2.3.2: "depltion"
Sec 3.1: "in circular aperture"
Sec 3.3: "apart from in"
Sec 3.4: "We thus again"
Sec 4.1: "compared include"
Sec 4.2.3: "has observed", "(, 200 and 50)"
Sec 4.4.1: "In our sample 8 of galaxies"
Sec 4.4.1: from (Murphy et al. 2011)

Sec 4.5: "the mm continuum appear"
Sec 5.2: "of Lbol (.... active)

AUTHORS: We have fixed these typos and grammar errors. We have taken extra care to proof read the manuscript and have tried to find any other typos or grammatical errors.